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THE INJURIOUS EFFECT

OF A

BLUE HEAT ON STEEL AND IRON.

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NOTE.

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THE INJURIOUS EFFECT OF A BLUE HEAT ON STEEL AND IRON.

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In spite of the many excellent qualities possessed by mild steel, and in spite of its extended use for ship-building and for marine boilers, there are still many engineers who consider it a treacherous material. They are able to adduce numerous instances in which steel plates and bars have failed, in their opinion at least, in a most unaccountable manner. In nearly all such cases a very cursory examination brings out the fact that the plates in question have been subjected to bending or hammering while hot, and there can be little doubt that, while they were being worked, these plates were at a blue heat, or, as smiths and boiler-makers call it, a black heat. It should by this time be well known that such treatment is the most injurious to which steel can possibly be subjected, and therefore that such failures cannot be properly designated as unaccountable.

Iron, as will be shown, possesses the same peculiarity; but as this material is very much less ductile than steel, similar failures are not so glaring, and when they occur are put down to bad quality.

Plates, both of iron and of steel, have, however, failed without this treatment, although the quality of the material was good, at least according to the usual tests.

The cases which have come under the author's immediate notice are as follows:

A steel plate which had been sheared after leaving the rolls, and had not subsequently been reheated, cracked in various places whilst it was being straightened (cold) between the rolls In another example the back-plate of a combustion-chamber of a marine boiler cracked while cold after the boiler had been out of use for about three months. On testing this steel plate it was found to stand the tensile and temper-bending tests satisfactorily. The iron plate was chipped and proved to be as ductile as could be wished, so that these cases do certainly appear to be mysterious.

It was not with the intention of discovering the reasons of these failures that the present series of experiments were undertaken, but in the hope of gaining additional information on the influence of a blue heat on steel. By an accident, a failure similar to the above occurred to one

of the test pieces. Although, unfortunately, the reasons for this failure are not as evident as could be wished, there are strong indications that one them has to be sought for in an accidental baking of the steel in a molder's core-drying oven; but this will be dealt with later on.

The numerous tests in connection with the subject of this paper consist mainly of bending and of tensile tests. The results of the latter are contained in the Appendix, Tables I and II, and in the diagrams. As they do not throw much light on the influence of blue heat, it does not appear necessary to give a very full explanation; they will therefore be dealt with first.

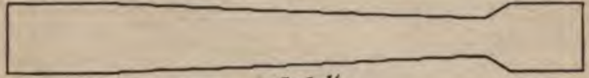
Table I contains the results of ordinary tensile tests on iron and steel. Several test pieces had, however, been previously subjected to mechanical operations.

It will be noticed that the iron, the hard steel, and the mild steel, all show an increase in their limits of elasticity if previously elongated. In some instances this limit rises above the original breaking-stress, although the ultimate breaking-stress is only slightly affected. The total elongation is reduced, while the contraction varies considerably. The rise in the limit of elasticity appears to be influenced both by the original amount of stretching and by the interval of time which elapses between the two tests. If the preliminary stretching is effected with the metal hot, the elastic limit rises higher than if the metal was cold.

A test-piece which had been shortened when cold showed a reduction of the elastic limit, while another piece which had been shortened when hot showed an increase. Two test-pieces which were stretched by flattening underwent a small ultimate elongation. The flattening had very little effect on the elastic limit and the ultimate breaking-stress of the cold-hammered piece, while the hot-hammered piece showed an increase both in the elastic limit and in the ultimate breaking-stress.

Some peculiar results, noticed during the testing of the above samples, suggested the idea that after the elastic limit has been exceeded test-pieces do not stretch uniformly throughout their length for every increase of stress. If this were true, then the ordinary stress-diagrams would not be of much value. In order to eliminate all influences which would produce incorrect results the test-pieces were shaped as in Fig. 1. Each section was very accurately measured before and after testing,

Fig. 1.



Scale ¼.

and the amount of local elongation was calculated from the amount of local contraction. As the stress per square inch varied for the different sections, the amount of elongation for each stress could be ascertained and plotted. The maximum stress was of course on the smallest

section, but as it was difficult to arrest the stretching when the highest load had been reached, the amount of this elongation is incorrect. This value and the maximum stresses are contained in Table II, while the stress diagrams are included in Figs. 2 to 7.

Fig. 2.

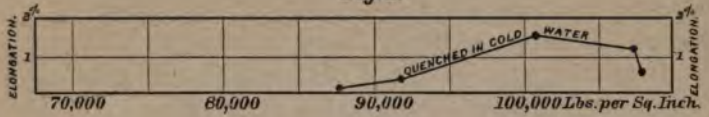
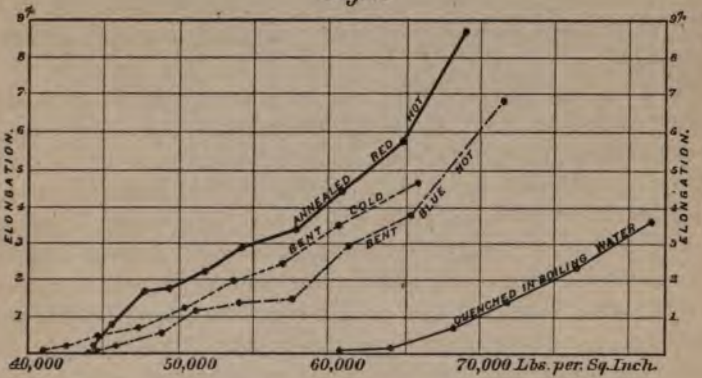


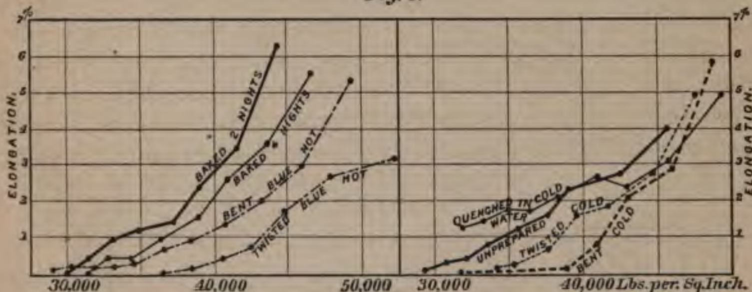
Fig. 3.



Siemens-Martin Steel. Medium Hard.

As will be seen, there is a marked difference in the same material under various conditions, but the results are too few for any conclusions to be drawn. Their value is also considerably diminished by want of homogeneity of at least one of the steels. It will be noticed that three lines in Fig. 5 run parallel with the base, which ought to be impossible.

Fig. 4.



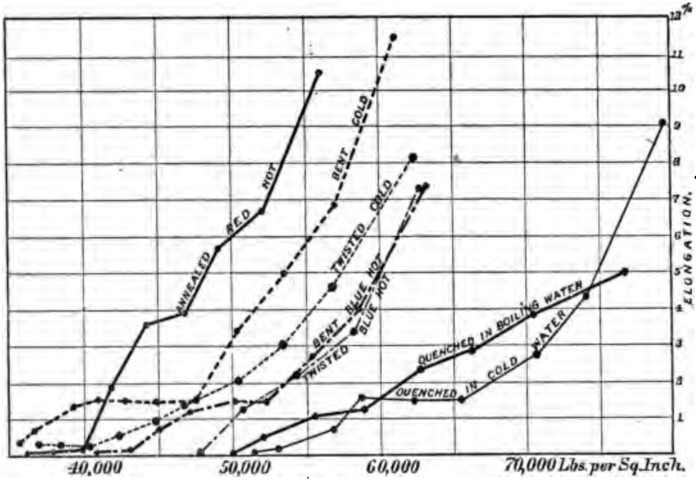
Lowmoor Iron.

Table III contains some tensile tests of steel at various temperatures.

Far more satisfactory results were obtained with the help of bending-tests. The earliest, which are contained in Table III, were made at a time when the peculiar influence of a blue heat was less understood

than it is now; they are only of value in so far as they embrace various qualities of steel and of iron. The same remarks apply to Table IV.

Fig. 5.



Siemens-Martin Steel. Mild.

In order to understand these and the following tables, it is necessary to explain that the test-pieces were never bent double, but first one way and then the other, till they broke.

Fig. 6.

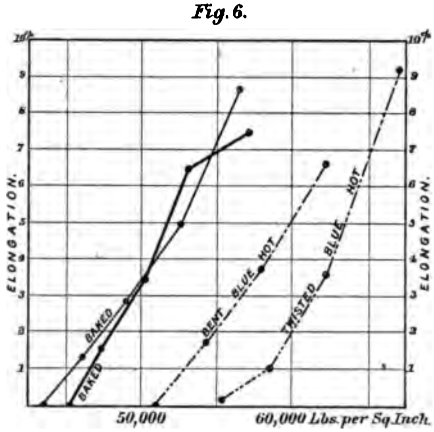
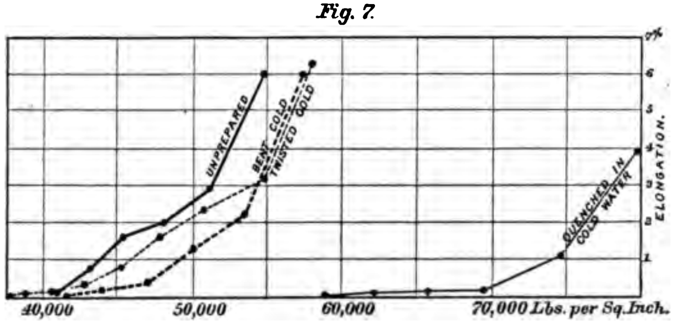


Fig. 7.



Siemens-Martin Steel. Very Mild.

In the tests of Table III this bending always amounted to 90°, and was carried out in a bending-machine driven by a belt. The tests in Table IV were made by clamping a test-piece between a steam-hammer and its anvil, and hammering the projecting end through an angle of about 45°; the steam-hammer was then lifted, the test-piece reversed, and again bent to an angle of about 45° the other way. This process was repeated till the test-piece broke. The number of bends can be looked upon as a fair measure of the ductility of the material. This method had two drawbacks: first, the angle was never properly measured; and secondly, there was great difficulty in always bending the same part of the test-piece. These difficulties were successfully overcome by using an anvil-mold, which was so arranged that the test-pieces could only be bent at one point, and not beyond an angle of 40°, while the radius of curvature was 1¼ inch. The last twelve tests of Table IV, and all those contained in Tables V to VIII, were made with this anvil mold.

In order to be quite clear, it must be explained that the expression "blue heat" is meant to include all those temperatures which produce discolorations of bright steel or of iron surfaces, ranging from light straw to blue. The temperatures corresponding with the various colors are:

	Fahrenheit. °
Straw color	470 = 243 C
Dark straw	480
Violet	490
Blue	500 to 600

In the Tables V to VIII the temperatures are pretty accurately defined by these various colors, while in the following remarks the expression "blue heat" will be used to include all these temperatures.

The first question to be considered is: Has a blue heat the same influence on iron as on steel while being bent? The answer is decidedly yes.

It will be noticed that, whereas the lowest number of bends which ⅜-inch steel would stand at a blue heat was two and a half, one and a half, and two and a half for three different qualities, Lowmoor iron of the same thickness only stood half a bend, or, more accurately, 24°, while the thin ($\frac{3}{16}$-inch) iron only stood three bends when hot, against twenty-one and more when cold.

As iron has been included in these tests, it was necessary to use it in thinner plates than the steel, for it had been found that, whereas some mild steel ⅜ inch thick would stand from twenty to thirty bends without breaking, Lowmoor iron of the same thickness would break at the third bend. (See Table IV.)

The next question was suggested by the failure of No. 13, Table III. Does bending permanently injure the ductility of steel and iron? This matter has been very exhaustively dealt with in Tables V and VI. It will be noticed that steel which had been cold, either once or twice, would stand almost as many subsequent bends as the original test-pieces.

But if the same material was bent once while blue-hot it lost a great deal of its ductility. Out of the twelve samples in which two preliminary bends were made with the test-piece when heated, nine broke with a single blow of a hammer, and the other three only stood one or two subsequent bends. The same results are to be found in Table VIII (mild steel). The thin Lowmoor iron does not break quite so easily, but stands about one-half the original number of bends, instead of breaking like glass or cast-iron.

The following table contains a few extracts and means:

	Medium hard steel. $\frac{3}{8}$ inch.	Mild steel. $\frac{3}{8}$ inch.	Very mild steel. $\frac{3}{8}$ inch.	Lowmoor iron. $\frac{3}{16}$ inch.
Unprepared or annealed	21	$12\frac{1}{2}$	26	20
Broken hot (blue)	$2\frac{1}{2}$	$1\frac{1}{2}$	$2\frac{1}{2}$	3
1 preliminary hot bend	3 mean	$2\frac{1}{3}$ mean	11 mean	12 mean
2 preliminary hot bends	$\frac{1}{3}$ mean	$\frac{1}{3}$ mean	$\frac{2}{3}$ mean	10 mean
1 preliminary cold bend	20	$9\frac{1}{2}$	....	....
2 preliminary cold bends	$19\frac{1}{2}$	$8\frac{1}{2}$	19	13
4 preliminary cold bends	....	....	13	11
8 preliminary cold bends	....	....	15	2

Besides subjecting the samples to preliminary bends, some pieces were twisted first one way and then another, both when cold and when hot, in order to ascertain the influence of shearing-stresses. Two twists of 45° in a length of 6 inches do not appear to have affected the ductility (as measured by subsequent cold-bending), no matter whether the test-pieces were twisted when cold or when hot. (See Tables V and VI.) Four twists of 90° in lengths of 6 inches had a marked influence, especially on the very mild steel, but less on the thin iron. The number of cold-bends of the steel were reduced from 26 to 15 after cold-twisting, and to 1 after hot-twisting. The number of cold-bends of the thin iron were raised from 20 to 23 after cold-twisting, and lowered to 7 after hot-twisting.

More decided results were obtained with test-pieces which had been slightly thinned down under a steam-hammer. Cold-hammering reduced the ductility from 26 to 21 in the case of the very mild steel, and from 20 to 9 in the case of the thin Lowmoor iron. Two samples of the same steel, after being hammered while hot, broke when cold with one hard blow of a hand-hammer (cystalline fracture), while the thin iron stood 6 and 7 bends.

All these results point unmistakably to the great danger which is incurred if iron or steel is worked at a blue heat. The difference between good iron and mild steel seems to be, that iron breaks more easily than steel while being bent when either hot or cold; that iron suffers more permanent injury than steel by cold working, but that if it has successfully withstood bending when hot, there is little probability of its flying to pieces when cold, as is almost sure to be the case with mild

It is a very common practice amongst boiler-makers to "take the chill out of a plate" if it requires a little setting, or to set a flanged-plate before it is cold. This is really nothing else than working it at a blue heat, and should not be allowed.

All hammering or bending of iron and steel should be avoided unless the metals are either cold or red-hot. Where this is impossible, and where the plate or bar has not broken while blue-hot, it should be subsequently annealed.

It is satisfactory to learn that, since the introduction of mild steel, a practice has been gaining ground amongst boiler-makers, which must have the effect of guarding against such failures, and should be encouraged. It consists in the cessation of work as soon as a plate, which has been red-hot, becomes so cool that the mark produced by rubbing a hammer-handle or other piece of wood over it will not glow. A plate which is not hot enough to produce this effect, yet too hot to be touched by hand, is most probably blue-hot, and should under no circumstances be hammered or bent.

The theory that local heating of a plate sets up strains which sometimes cause failures, and which is generally accepted as correct, does not appear to have any foundation in fact. It is, however, doubtful whether the proposal to locally reheat a plate, which has been worked hot, in order to anneal this part, should be carried out. This question is dealt with in Tables V to VIII. Several test-pieces were made red-hot or blue-hot, and were then slowly cooled by holding one of their edges in cold water. As might have been expected, the medium hard steel lost much of its ductility. The other steels and the iron were not much affected, as will be seen from the following short table of the number of bends sustained by the samples:

	Medium hard steel.	Mild steel.	Very mild steel.	Low-moor iron.
Unprepared or annealed	21	12½	26	20
Quenched red-hot in boiling water	24	10	......	......
Quenched red-hot in cold water	1	10	19	20
Red-hot, quenched edge in cold water	3	8	25	27
Blue-hot, quenched edge in cold water	3	6½	19, 19	21, 14

Before concluding it will be necessary to revert to the peculiar failure of one of the tensile test-pieces of the medium hard steel, which on being straightened edgeways broke with a crystalline fracture. Of these pieces one was then bent in the anvil-mold, but broke with a partly crystalline fracture at ½ bend instead of standing 21 bends. (See Table V, Nos. 7 and 8.) On inquiry it was found that instead of annealing all the test-pieces of Tables V and VI, in a plate-heating furnace, they had been baked in a molder's core-drying oven, and it seems that the temperature of these ovens is about equal to a blue heat. This baking did not at first appear to be an adequate reason for the

failure, for all plates which are annealed must pass through a blue heat; but on comparing all the bending-tests of Table V a peculiarity showed itself which made it appear probable that the prolonged heating had spoilt the steel. Most of the test pieces had been made red-hot subsequent to the baking, and either cooled slowly or in water; but a few were never thus heated to redness. It is interesting to compare these with some which had been heated, but otherwise treated in the same way. (Compare Nos. 3, 7, 8, 14, 16, and 20 with Nos. 2, 5, 10, 18, and 19.) The former broke after standing 3, 0, ½, 1, 3, and half bends, while the latter stood 21, 22½, 24, 22, and 23 bends. The means are 1⅓ and 22½ bends. This difference was so glaring that Table VI was analyzed in the same way but the result was not so marked. The means are respectively 8½ and 10 under the same conditions. Further experiments on this subject will be found in Tables VII and VIII, Nos. 1 to 12, but the results are as undecided as the last. The averages come to 16 and 20 for iron, and to 22 and 26 for very mild steel.

At first sight it might appear that the medium hard steel plate had by some accident been plunged in cold water while it was still red-hot; but a little reflection will show that, if it had been thus hardened and was so brittle as to break with one blow of a hammer, it could not have been sheared into many small pieces without at least one of them breaking. Unfortunately all the test-pieces of this steel have been broken; otherwise the question, whether one quality of steel is and another is not affected by long continued exposure to a blue heat, might have been investigated. Should it ultimately be shown that the former supposition is correct, then it would be necessary either to invent some test which would readily detect such steel, or else to take great care not to expose any steel for prolonged periods to a blue heat.

It must be pointed out that such baking may easily occur if large quantities of iron or steel are annealed in a single furnace. It can also take place in the rolling-mills, especially if there is a deficiency of floor-space. Under such circumstances, it frequently happens that twenty or thirty hot-plates are stacked one upon another. Each plate leaves the rolls at a dull red heat. By the time that the first plate has cooled to a blue heat the next is thrown on it and maintains the temperature of the bottom one. The third dull red-hot plate is thrown on the others when the second one has also just reached a blue heat. This process is repeated till the pile is complete and blue-hot throughout, at which temperature it remains for a considerable time. If the quality of one of the plates is such as to be affected by this sort of annealing, then there is little doubt that it will have been injured.

It would be of great interest if this question could be further investigated, and also if steel manufacturers could ascertain whether every quality of steel is made permanently brittle by being worked at a blue heat, or whether this is due to the various impurities contained in it.

It should be remembered that iron, though less pure than mild steel, contains the foreign substances in the form of finely-divided slag, and that in good brands the iron layers are probably purer than even the purest mild steel. A very marked difference between iron and steel is the presence of manganese in the latter. The admixture is, of course, necessary on account of the red-shortness of pure Bessemer or Siemens ingot-iron; but it is also possible that blue-shortness (if this expression is permissible) is due to the presence of this substance. On the other hand it must not be forgotten that all tool-steel, which should contain no manganese, is strongly affected in its hardness by the temperatures embraced under a blue heat. This would show that manganese does not influence this quality.

Whatever be the cause of the peculiarity, and whether it will ever be possible to produce steel and iron which are not blue-short, it is at least clear that the experiments contained in this paper show how steel can with certainty be made permanently brittle. In practice everything should therefore be done to avoid any treatments at all involving the working of iron and of steel while blue-hot.

In conclusion, the author wishes to tender his thanks to the manufacturers who have kindly supplied him with the materials, and to Prof. Alex. B. W. Kennedy, M. Inst. C.E., and Messrs. J. Stewart & Son, for the use of their appliances.

The paper is accompanied by several diagrams, from which the figures in the text have been prepared.

APPENDIX.

TABLE I.—*Repeated tensile tests of iron and hard and mild steel.*

	No.	Materials.	Particulars.	Elastic limits.	Preliminary. Stress.	Preliminary. Elongation.	Ultimate. Stress.	Ultimate. Elongation.	Ultimate. Contraction.
				Tons per sq. inch.	*Tons per sq. inch.*	*Per cent. in 8 inch.*	*Tons per sq. inch.*	*Per cent. in 8 inch.*	*Per cent.*
	1	Iron	C.	16.1			23.5	12.5	(?)
	2	....do	Elongated twice	14.8	21.0	4.2			
				23.5			26.0	12.8	(?)
	3	....do	Elongated four times; intervals 3, 5, and 21 days	(?)	(?)	4.1			
				19.1	22.7	7.2			
				(?)	(?)	12.1			
				29.1			30.0	14.2	15.5
Cut from one plate.	4	Hard steel.	Crossways C.	19.4			40.5	17.0	(?)
	5	....do	Lengthways L.	20.0			40.5	20.0	(?)
	6	....do	Elongated twice C.	19.3	35.2	5.0			
				39.8			40.3	18.1	(?)
	7	.. do	Elongated twice L.	20.0	35.7	5.0			
				43.2			43.5	19.7	(?)
	8	....do	Quenched red-hot in cold water	51.5			53.5	.0	(?)
	9	....do	Tested hot (blue to straw) C.	15.9			51.0	13.0	58.3
	10	....do	Tested hot L (straw to pale)	17 0			51.0	11.0	(?)
Cut from three plates.	I 11	Mild steel.	Annealed red-hot	19.3			29.7	20.1	58.3
	II 12	....do	Annealed red-hot, elongated twice; interval, 10 days	16.3	24.6	5.2			
				28.5			29.8	17.0	44.0
	III 13	....do	......do	18.4	26.0	0.95			
				28.0			28.6	13.0	(?)
	III 14	....do	Annealed red-hot; elongated twice; interval, 15 days	16.6	28.8	15.3			
				32.7			32.7	19.0	(?)
	II 15	....do	Annealed red-hot; elongated 3 times; intervals, 1 day and 8 days	15.6	23.4	5.4			
				26.9	27.6	7.8			
				29.1			30.2	14.5	53.3
	I 16	....do	Quenched red-hot in cold water	14.7			40.0	13.2	43.8
	I 17	....do	Quenched red-hot in cold water; elongated 3 times; intervals, 3 and 8 days	9.3	30.2	2.9			
				33.2	37.0	6.7			
				38.3			43.2	10.4	

TABLE I.—*Repeated tensile tests of iron and hard and mild steel*—Continued.

	No.	Materials.	Particulars.	Elastic limits.	Preliminary.		Ultimate.		
					Stress.	Elongation.	Stress.	Elongation.	Contraction.
				Tons per sq. inch.	*Tons per sq. inch.*	*Per cent. in 8 inch.*	*Tons per sq. inch.*	*Per cent. in 8 inch.*	*Per cent.*
Cut from three plates.	II 18	....do.....	Elongated while blue-hot; retested cold ..	10.0	27.7	4.9	......	....	.....
				30.7	....	....	33.0	16.0	50.6
	II 19	....do.....	Elongated while blue-hot; retested twice while cold	(?)	(?)	4.5	....	....	.....
				30.2	32.3	7.6	....	....	.....
				34.3	....	....	35.6	12.8	43.0
	II 20	....do.....	Shortened cold under steam-hammer......	Shortened.		4.7	....	....	.....
				11.8	....	....	30.7	7.2	55.0
	II 21	....do....	Shortened hot (blue) under steam-hammer	Shortened.		3.6	....	....	.....
				24.2	....	....	33.5	8.6	48.6
	I 22	...do.....	Flattened cold on a smith's anvil........	Flattened.		1.5	....	....	.....
				20.5	....	....	32.0	12.9	65.3
	I 23	....do.....	Flattened hot (blue) on smith's anvil	Flattened.		1.5	....	....	.....
				25.0	....	....	36.3	9.5	40.5

TABLE II.—*Tensile tests made with taper test-pieces (diagram) in connection with the bending-tests of Tables V to VIII.*

No.	Particulars.	Elastic limits.	Highest tensile strength.		Local elongation.
		Tons per sq. inch.	*Tons per sq. inch.*	*Lbs. per sq. inch.*	*Per cent.*
	Siemens-Martin steel. Medium hard.				
	35.7 tons=82,600 pounds.				
		About.			
1	Annealed red-hot	19.6	32.5	72,900	11.54
2	Quenched red-hot in boiling water	27.2	38.5	86,300	5.83
3[1]	Quenched red-hot in cold water.........................	39.1	48.2	108,000	1.17
4	Annealed red-hot; bent cold to a 15″-radius and flattened.	18.3	31.7	71,000	7.22
5	Annealed red-hot; bent hot (violet to straw) and flattened	19.6	33.9	76,000	16.73
	Siemens-Martin steel. Mild.				
	28.2 tons=63,100 pounds.				
6	Annealed red-hot	16.1	26.5	59,400	34.72
7	Quenched red-hot in boiling water......................	22.3	37.0	82,800	16.46
8	Quenched red-hot in cold water	23.0	36.5	81,900	12.47
9	Annealed red-hot; bent cold to a 15″-radius and flattened	15.6	29.0	65,000	24.26
10	Annealed red hot; bent hot (blue to dark straw) and flattened	18.3	29.9	67,000	20.16
11	Annealed red-hot; twisted cold (90° in 12″ and back) ..	(?)	29.5	66,100	16.83
12	Annealed red-hot; twisted hot (blue to violet) and back.	21.2	30.3	67,900	22.44
	Siemens-Martin steel. Very mild.				
	23.4 tons=51,400 pounds.				
13	Unprepared	18.3	27.2	61,000	24.80
14	Quenched red-hot in cold water	26.3	39.0	87,400	19.42
15	Annealed blue-hot two nights............................	19.3	27.4	61,300	20.61
16	Annealed blue-hot four nights	20.1	27.0	60,500	10.40

TABLE II.—*Tensile tests made with taper test-pieces, &c.*—Continued.

No.	Particulars.	Elastic limits.	Highest tensile stretch.		Local elongation.
	Siemens-Martin steel. Very mild—Continued.	*Tons per sq. inch.*	*Tons per sq. inch.*	*Lbs. per sq. inch.*	*Per cent.*
	23.4=51,400 pounds.	About.			
17	Bent cold to a 15″-radius and flattened	18.5	28.1	63,000	17.98
18	Bent hot (blue to straw) and flattened	22.8	29.5	66,000	29.40
19	Twisted cold (90° in 12″ and back)	17.0	28.2	63,100	16.27
20	Twisted hot (blue to dark straw) and back	24.6	31.7	71,000	27.90
	Lowmoor iron.				
21	Unprepared	12.5	22.1	49,600	6.67
22	Quenched red-hot in cold water	(?)	24.5	54,800	7.02
23	Annealed blue-hot two nights	13.4	21.7	47,600	18.71
24	Annealed blue-hot four nights	14.1	22.1	49,900	14.16
25	Bent cold to a radius of 15″ and flattened	14.1	23.2	52,100	18.78
26	Bent hot (blue to straw) and flattened	12.5	23.6	52,900	7.06
27	Twisted cold (90° in 12″ and back)	15.2	23.2	52,000	8.46
28	Twisted hot (blue to dark straw) and back	16.3	24.6	55,000	16.33

[1] No. 3 broke at a point where the stress was 93,500 pounds, and the elongation 0.38 per cent.

TABLE III.—*Various tensile and bending tests of iron and mild and hard steel.*

Defective steel garboard plate, ⅝ inch thick.

No.
1. Tensile test 28.8 tons per square inch; 24 per cent. elongation in 8 inches.
2. Tensile-test 28.9 tons per square inch; 23 per cent. elongation in 8 inches.
3. Tensile-test between rivet-holes 27.8 tons per square inch; 20 per cent. elongation.
4. Tensile-test between rivet-holes annealed 28.8 tons per square inch; 25 per cent. elongation.
5. Quenched red-hot in cold water; bent double.
6. Quenched red-hot in cold water; bent (by machine), 4 angles of .... 90°
7. Unprepared; bent (by machine), 2 angles of .... 90°
8. Annealed; bent (by machine), 4 angles of .... 90°
9. Punched before shearing; broke without bending .... 0°
10. Punched after shearing; broke without bending .... 0°
    Other end made red-hot bent to right-angle through punched hole; cooled slowly and bent double .... 90°
11. Punched before shearing, then annealed; bent (machine) .... 90°
12. Punched before shearing, then annealed; bent (machine) .... 100°
    Other end bent while blue-hot, broke through rivet hole .... 45°
13. Punched after shearing and annealing; not broken .... 100°
    Other end bent to 30° while blue-hot; cooled slowly for an hour, broke with one blow of a hammer .... 0°
14. Bent through original rivet-holes; result good.
15. Bent through original rivet-holes; result good.

Mild Siemens-Martin steel, ⅜ inch thick.

16. Tensile-test 25 tons per square inch; 22 per cent. elongation.
17. Quenched red-hot in cold water; bent double (steam-hammer) .... 180°
18. Quenched red-hot in cold water; bent (machine), 6 angles of .... 90°
19. Quenched red-hot in cold water; bent (machine), 6 angles of .... 90°
20. Annealed red-hot; bent (machine), 5 angles of .... 90°
21. Bent blue-hot; broke .... 30°
22. Bent blue-hot to an angle of 15° when cold; broke .... 90°

Hard Siemens-Martin steel, $\frac{7}{16}$ inch thick.

23. Tensile-test 37 tons per square inch; 20 per cent. elongation.
24. Annealed red-hot; bent (machine), 6 angles .... 90°
25. Quenched red-hot in cold water; bent (machine), 2 angles.......... 90°
26. Annealed blue-hot; bent (machine) cold, 2½ angles.......... 90°
27. Bent blue-hot; bent (machine) hot, 2½ angles .... 90°

Hard spring steel, ⅜ inch thick.

28. Annealed red-hot; bent (machine), 2¼ angles .... 90°
29. Annealed blue-hot; bent (machine) cold, 1¼ angles .... 90°
30. Bent blue-hot; bent (machine) hot, 2½ angles .... 90°
31. Bent straw-hot; bent (machine) hot, 2½ angles .... 90°

Iron equal to Lowmoor, $\frac{7}{16}$ inch thick.

32. Unprepared, bent (machine), 2 angles.......... 90°
33. Unprepared, bent (machine), 2 angles.......... 45°
34. Quenched red-hot in cold water, bent (machine) cold, 4 angles .... 45°
35. Bent blue-hot, bent (machine) hot, 2 angles .... 45°
36. Bent straw-hot, bent (machine) hot, 2 angles .... 45°

		Breaking-stress.	Elongation
37. Tensile test of steel, cold..........	L[1]	28.1 tons.	23 per cent.
38. Tensile test of steel, cold..........	C	29.6 tons.	23 per cent.
39. Tensile test of steel, blue-hot, straw-fracture	L	36.2 tons.	15 per cent.
40. Tensile test of steel, blue-hot, straw-fracture	C	35.5 tons.	15½ per cent.
41. Tensile test of steel, dark red, blue fracture	L	27.8 tons.	21 per cent.
42. Tensile test of steel, dark red, black fracture..........	C	18.4 tons.	25 per cent.
43. Tensile test of best iron, cold..........	L	22.2 tons.	24 per cent.
44. Tensile test of best iron, cold..........	C	23.0 tons.	20½ per cent.
45. Tensile test of best iron, blue-hot, straw fracture..........	L	31.1 tons.	16½ per cent.
46. Tensile test of best iron, blue-hot, straw fracture..........	C	29,2 tons.	10 per cen

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TABLE IV.—*Bending-tests of various steels and irons.*

[The bending was done with a sledge-hammer. The bends were approximately equal to 45°.]

Mild steel, ⅜ inch thick.

No.		No. of bends.
1.	Unprepared	10
2.	Annealed red-hot	21
3.	Quenched red-hot in boiling water..........	14
4.	Quenched red-hot in tepid water (82° Fahrenheit)..........	6
5.	Quenched red-hot in ice-cold water	2
6.	Red hot cooled slowly to straw, bent hot..........	5
7.	Bent hot (pigeon gray)..........	6
8.	Bent hot (blue)..........	6
9.	Bent hot (violet)	2
10.	Bent hot (straw)	4
11.	Bent twice while hot (blue), quenched in water, bent cold[2]	2½
12.	Bent twice while hot (straw), cooled slowly, bent cold	1

Mild steel, ⅜ inch thick.

13.	Unprepared..........	16
14.	Quenched red-hot in cold water	16
15.	Bent hot (pigeon gray), fracture straw..........	4
16.	Bent hot (blue), fracture dark straw..........	2½
17.	Unprepared; bent double.	
18.	Quenched red-hot in cold water; bent double.	

[1]L and C signify lengthwise and crosswise. [2]Broke short 1 inch from original bend.

Mild steel, ⅜ inch thick.

	No. of bends.
19. Annealed red-hot	10½
20. Annealed dark red-hot	10
21. Quenched red-hot in cold water	10
22. Bent hot (blue)	2½
23. Bent hot (violet)	2½
24. Bent twice while hot (blue), cooled slowly (bending reversed)	1½
25. Bent twice while hot (blue), cooled slowly (bending continued)	2½
26. Unprepared	6½
27. Annealed red-hot	10½
28. Quenched red-hot in cold water	10½
29. Unprepared, bent twice; after 2 weeks broke	5
30. Unprepared, bent twice; after 6 weeks broke	2
31. Unprepared, bent IV; after 6 weeks broke	1
32. Unprepared, bent IV; after 6 weeks broke	4
33. Annealed red-hot, bent IV; after 6 weeks broke	9
34. Quenched red-hot, bent II; after 6 weeks broke	4
35. Quenched red-hot, bent IV; after 6 weeks broke	7
36. Bent hot (dark straw)	1½

Basic steel, mild quality, ⅜ inch thick.

37. Unprepared	16
38. Quenched red-hot in cold water	16
39. Bent hot (blue), fracture straw	4
40. Bent hot (blue), fracture dark straw	2½

Basic steel, hard quality, ⅜ inch thick.

41. Unprepared	12
42. Quenched red-hot in cold water	1
43. Bent hot (blue), fracture dark straw	4

Lowmoor iron, ⅜ inch thick.

Bent in anvil mold to 40° with a radius of 1¾ inch.

	Cracked.	Broke.
44. Lengthways, unprepared	2	3 (n. b.)
45. Lengthways, unprepared	2	3 (n. b.)
46. Lengthways, bent hot (blue), broke with one hard blow	..	0
47. Lengthways, bent hot (straw), broke with one hard blow	..	0
48. Lengthways, bent hot (blue), light hammering	26°	.. (n. b.)
49. Lengthways, bent hot (straw), light hammering	37°	.. (n. b.)
50. Crossways, unprepared	2½	3 (n. b.)
51. Crossways, unprepared	2½	3 (n. b.)
52. Crossways, bent hot (blue), light hammering	33°	.. (n. b.)
53. Crossways, bent hot (violet), light hammering	33	.. (n. b.)
54. Crossways, bent hot (straw), light hammering	29	.. (n. b.)
55. Crossways, bent hot (light straw), light hammering	24	.. (n. b.)

NOTE.—(n. b) signifies that the test-piece did not break in two.
The Roman numerals denote the number of preliminary bends.

TABLE V.—*Bending-tests of Siemens-Martin steel. Medium hard, 35.7 tons per square inch.*[1]

[Dimensions of test-pieces, 6 inches × 1½ inches × 0.34 inch. Sheared from one plate. Every recorded bend amounts to 40°, curvature 1¾-inch radius. Every test-piece was baked in a molder's core-drying oven for two nights.]

No.	Particulars.	No. of bends.	
		Cracked.	Broke.
	Annealed at a red or blue heat.		
1	6 D.[2] Unprepared except by baking	....	15
2	6 D. Annealed r.d-hot	17	21
3	6 D. Annealed violet	1	3
4	6 D. Annealed violet 35 D	3	5
5	6 D. Annealed red-hot; 5 D. Annealed blue	19	22½
6	5 D. Quenched red-hot in boiling water; 1 D. Annealed violet 8 D	17	23 (n.b.)
7	1 D. Bent edgeways; broke without bending	....	0
8	1 D. Bent flatways	....	½
	Red or blue hot quenched in water.		
9	5 D. Quenched red in cold water	....	1 (n b.)
10	5 D. Quenched red in boiling water; 1 D	23	24
11	5 D. Quenched red in boiling water; quenched straw in cold water; 1 D	16	17
12	5 D. Quenched red in boiling water; quenched straw in cold water; 1 D	....	26
13	5 D. Quenched red in boiling water; quenched straw in cold water; 6 D	18	19
14	6 D. Quenched violet in cold water; 7 D	....	1
	Edges quenched in cold water.		
15	6 D. Quenched dark straw in cold water; 35 D. Red-hot, quenched edge in cold water	1	3
16	6 D. Quenched violet in cold water; 35 D. Blue-hot, quenched edge in cold water	1	3
	Broken while blue-hot.		
17	6 D. Blue-hot; fracture straw	....	2½
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Twist in 6 inches, 45° one way and back.		
18	6 D. Annealed red-hot, twisted cold; 7 D	19	22
19	6 D. Annealed red-hot, quenched red in boiling water, twisted cold; 7 D	17	23 (n.b.)
20	6 D. Annealed blue, twisted cold; 7 D	....	½
21	6 D. Annealed red-hot, twisted hot (blue); 7 D	17	21
	BENT COLD AFTER PRELIMINARY BENDING.		
	One preliminary bend.		
22	6 D. Annealed red-hot, I cold bend; 7 D	14	20
23	5 D. Annealed red-hot, 1 D. I cold bend; 35 D	18	20 (n.b.)
24	5 D. Quenched red hot in boiling water; 35 D. I cold bend; 1 D	21	25 (n.b.)
25	5 D. Quenched red-hot in boiling water; 1 D. I cold bend; 7 D	10	16 (n.b.)
26	5 D. Quenched red-hot in boiling water; 1 D. I cold bend; 35 D	12	16 (n.b.)
27	5 D. I hot bend (dark straw); 1 D	5	5½
28	6 D. I hot bend (light straw); 7 D	1	2
29	6 D. I hot bend (blue); 35 D	....	1½

[1] See Table II and diagrams for tensile tests.

[2] D attached to a numeral denotes an interval of a certain number of days; (n.b.) signifies that the test-piece did not break in two at the last bend; the Roman numerals I and II denote one or two preliminary bends.

TABLE V.—*Bending-tests of Siemens-Martin steel, &c.*—Continued.

No.	Particulars.	No. of bends.	
		Cracked.	Broke.
	BENT COLD AFTER PRELIMINARY BENDING—continued.		
	Two preliminary bends.		
30	6 D. Annealed red-hot; 5 D. II cold bends; 1 D. Bending continued	15	18½
31	6 D. Annealed red-hot; II cold bends; 7 D. Bending continued	17	21
32	5 D. Annealed red-hot; II cold bends 35 D. Bending continued	17	20 (n.b.)
33	5 D. Annealed red-hot; 5 D. II cold bends; 1 D. Bending reversed	13	17 (n.b.)
34	5 D. Annealed red-hot; II cold bends; 7 D. Bending reversed	15	19
35	5 D. Annealed red-hot; II cold bends; 35 D. Bending reversed	17	21 (n.b.)
36	5 D. Quenched red-hot in boiling water; 35 D. II cold bends; 1 D. Bending continued	15	19
37	5 D. Quenched red-hot in boiling water; 1 D. II cold bends; 7 D. Bending continued	15	21 (n.b.)
38	5 D. Quenched red-hot in boiling water; 1 D. II cold bends; 35 D. Bending continued	18	22
39	5 D. Quenched red-hot in boiling water; 35 D. II cold bends; 1 D. Bending reversed	17	20
40	5 D. Quenched red-hot in boiling water; 1 D. II cold bends; 7 D. Bending reversed	13	15
41	5 D. Quenched red-hot in boiling water; 1 D. II cold bends; 35 D. Bending reversed	....	17½
42	5 D. II hot bends (straw); 1 D. Bending continued	....	1
43	5 D. II hot bends (light straw); 7 D. Bending continued	....	0
44	6 D. II hot bends (dark straw); 35 D. Bending continued	....	1
45	5 D. II hot bends (light straw); 1 D. Bending reversed	....	0
46	6 D. II hot bends (straw); 7 D. Bending reversed	....	0
47	6 D. II hot bends (dark straw); 35 D. Bending reversed	....	0

TABLE VI.—*Bending-tests of Siemens-Martin steel. Mild, 28.2 tons per square inch.*[1]

[Dimensions of test-pieces, 6 inches × 1½ inches × 0.35 inch. Sheared from one plate. Every recorded bend amounts to 40°, curvature 1¾ inch radius. Every test-piece was baked in a molder's coredrying oven for two nights.]

No.	Particulars.	Number of bends.	
		Cracked.	Broke.
	Annealed at a red or blue heat.		
1	6 D. Unprepared except by baking	3	6
2	6 D. Annealed red-hot	9	12½
3	6 D Annealed violet	3	8
4	6 D. Annealed violet; 35 D	3	7½
5	6 D. Annealed red-hot; 35 D. Annealed straw; 1 D	3	7 (n. b.)
6	5 D. Quenched red-hot in cold water; 1 D. Annealed violet; 7 D	7	9
	Red or blue hot quenched in water.		
7	5 D. Quenched red in cold water; 1 D	7	10
8	5 D. Annealed red-hot; 35 D. Boiling water	....	10

[1] See Table II and diagrams for tensile tests.

TABLE VI.—*Bending-tests of Siemens-Martin steel, &c.*—Continued.

No.	Particulars.	Number of bends. Cracked.	Broke.
	Red or blue hot quenched in water—Continued.		
9	5 D. Annealed red-hot in cold water; 1 D. Quenched straw in cold water; 1 D.	....	10
10	5 D. Quenched red in cold water; 1 D. Quenched dark straw in cold water; 7 D	7	9 (n. b.(
11	5 D. Quenched red in cold water; quenched straw in cold water; 36 D.	12	12½
12	6 D. Quenched violet in cold water	3	6
	Edges quenched in cold water.		
13	6 D. Quenched dark straw in cold water; 35 D. Red hot, quenched edge in cold water	5	8 (n. b.)
14	6 D. Quenched violet in cold water; 35 D. Violet, quenched edge in cold water	3	6½ (n. b.)
	Broken while blue-hot.		
15	6 D. Blue-hot; fracture light blue	....	1½
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Twists in 6 inches, 45° one way and back.		
16	5 D. Annealed red-hot; 1 D. Twisted cold; 7 D.	3	11
17	5 D. Annealed red-hot; 1 D. Quenched red in cold water, twisted cold; 7 D	5	7
18	6 D. Annealed violet, twisted cold; 7 D	9	11
19	5 D. Annealed red-hot; 1 D. Twisted hot (blue); 7 D	5	9
	BENT COLD AFTED PRELIMINARY BENDING.		
	One preliminary bend.		
20	5 D. Annealed red-hot; 1 D. I cold bend; 7 D	....	10
21	5 D. Annealed red-hot; 1 D. I cold bend; 35 D	....	9
22	5 D. Quenched red-hot in cold water; 6 D. I cold bend; 1 D	1	4
23	5 D. Quenched red-hot in cold water; 1 D. I cold bend; 7 D	4	6
24	5 D. Quenched red-hot in cold water; 1 D. I cold bend; 35 D	....	5½
25	5 D. I hot bend (gray cooled to blue); 1 D	....	1
26	5 D. I hot bend (straw); 7 D	3	4
27	6 D. I hot bend (light straw); 35 D	1	1½
	Two preliminary bends.		
28	5 D. Annealed red-hot; 35 D. II cold bends; 1 D. Bending continued	7	9 (n. b.)
29	5 D. Annealed red-hot; 1 D. II cold bends; 7 D. Bending continued	7	9
30	5 D. Annealed red-hot; 1 D. II cold bends; 35 D. Bending continued	....	4½ (n. b.)
31	6 D. Annealed red-hot; 35 D. II cold bends; 1 D. Bending reversed	3	7
32	5 D. Annealed red-hot; 1 D. II cold bends; 7 D. Bending reversed	7	11
33	6 D. Annealed red-hot; II cold bends; 35 D. Bending reversed	7	9½
34	5 D. Quenched red in cold water; 35 D. II cold bends; 1 D. Bending continued	1	5
35	5 D. Quenchod red in cold water; 1 D. II cold bends; 7 D. Bending continued	....	7 (n. b.)
36	5 D. Quenched red in cold water; 1 D. II cold bends; 35 D. Bending continued	....	4½
37	5 D. Quenched red in cold water; 6 D. II cold bends; 1 D. Bending reversed	3	5½
38	5 D. Quenched red in cold water; 1 D. II cold bends; 7 D. Bending reversed	7	12

TABLE VI.—*Bending-tests of Siemens-Martin steel, &c.*—Continued.

No.	Particulars.	Number of bends.	
		Cracked.	Broke.
	BENT COLD AFTER PRELIMINARY BENDING—continued. *Two preliminary bends*—Continued.		
39	5 D. Quenched red in cold water; 1 D. II cold bends; 35 D. Bending reversed	3	5
40	5 D. II hot bends (blue); 1 D. Bending continued	....	0
41	6 D. II hot bends (dark straw); 7 D. Bending continued	....	0
42	6 D. II hot bends (violet); 35 D. Bending continued	....	0
43	5 D. II hot bends (dark straw); 1 D. Bending reversed	....	2
44	6 D. II hot bends (dark straw); 7 D. Bending reversed	....	0
45	6 D. II hot bends (straw); 35 D. Bending reversed	....	0

TABLE VII.—*Bending-tests of Lowmoor iron.*[1]

[Dimensions of test-pieces: 6 inches × 1½ inches × 0.19 inch. Sheared from one plate. Every recorded bend amounted to 40°. Curvature 1¾-inch radius.]

No.	Particulars.	Number of bends.	
		Cracked.	Broke.
	Annealed at a red and blue heat.		
1	Unprepared	13	21 (n. b.)
2	Annealed red-hot	....	19
3	Annealed blue; 7 D	6	15 (n. b.)
4	Annealed blue; 7 D	11	19 (n. b.)
5	Annealed violet; 7 D	11	14 (n. b.)
6	Baked for 2 nights; 5 D	9	18 (n. b.)
7	Baked for 4 nights	7	15 (n. b.)
8	Baked for 8 nights	9	17 (n. b.)
9	Baked for 4 nights; 3 D. Annealed dark straw; 7 D	9	15 (n. b.)
10	Baked for 8 nights. Annealed blue; 3 D	11	15 (n. b.)
11	Baked for 13 nights. Straw to violet; 4 D	7	18 (n. b.)
12	Quenched red in cold water; 4 D. Baked one month; 28 D	23	29
13	Quenched blue in cold water; 4 D. Baked one month; 28 D	6	15
	Red and blue hot, quenched in cold water.		
14	Quenched red-hot; 4 D	17	20
15	Quenched violet; 7 D	7	17 (n. b.)
16	Red-hot, quenched edge; 4 D	20	27 (n. b.)
17	Pigeon-gray, quenched edge; 3 D	11	21 (n. b.)
18	Violet, quenched edge; 3 D	7	14 (n. b.)
	Broken while blue-hot.		
19	Blue-hot; fracture blue	....	5 (n. b.)
20	Blue-hot; fracture straw	....	4½
21	Violet; fracture dark straw	....	3
22	Dark straw; fracture light straw	....	5
23	Quenched red in cold water; 4 D. Violet; fracture straw	....	7
	Flattened hot and cold under a steam hammer.		
24	Reduced 16 per cent. cold; 3 D	....	9 (n. b.)
25	Reduced 12 per cent. hot (blue); 3 D	3	6 (n. b.)
26	Reduced 6 per cent. hot (violet); 3 D	....	7

[1] See Table II and diagrams for tensile tests.

TABLE VII.—*Bending-tests of Lowmoor iron*—Continued.

No.	Particulars.	Number of bends.	
		Cracked.	Broke.
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Four twists of 90° in a length of 6 inches.		
27	Quenched red in cold water; 4 D. Twisted cold	19	23 (n. b.)
28	Quenched violet in cold water; 4 D. Twisted cold	....	15 (n. b.)
29	Twisted hot (blue to light straw)	....	7 (n. b.)
	BENT COLD AFTER PRELIMINARY BENDING.		
	One preliminary hot bend.		
30	I hot bend (blue); 7 D	12	16 (n. b.)
31	I hot bend (dark straw); 7 D	8	11 (n. b.)
32	I hot bend (light straw); 7 D	4	10 (n. b.)
	Two preliminary hot bends.		
33	II hot bends (blue); 7 D	6	10½ (n. b.)
34	II hot bends (dark straw); 7 D	7	12½ (n. b.)
35	II hot bends (light straw); 7 D	3	6 (n. b.)
	Several preliminary cold bends.		
36	II cold bends, annealed violet; 4 D	6	13 (n. b.)
37	II cold bends, quenched blue in cold water; 4 D	8	13 (n. b.)
38	Quenched red in cold water; 4 D. IV cold bends; 3 D	....	11 (n. b.)
39	VIII cold bends, annealed blue	4	7 (n. b.)
40	VIII cold bends, quenched dark straw in cold water; 4 D	4	7 (n. b.)
41	VIII cold bends; 50 D	1	2 (n. b.)
42	Annealed blue; 7 D. VIII cold bends; 50 D	3	6 (n. b.)

TABLE VIII.—*Bending-tests of Siemens-Martin steel. Very mild, 23.4 tons per square inch.*[1]

[Dimensions of test-pieces: 6 inches × 1½ inches × 0.40 inch. Sheared from one plate. Every recorded bend amounted to 40°. Curvature, 1¾-inch radius.]

No.	Particulars.	Number of bends.	
		Cracked.	Broke.
	Annealed at a red and blue heat.		
1	Unprepared	19	27 (n. b.)
2	Annealed red-hot	19	25
3	Annealed blue; 7 D	15	19 (n. b.)
4	Annealed violet; 7 D	20	24 (n. b.)
5	Annealed dark straw; 7 D	11	27 (n. b.)
6	Annealed dark straw; 7 D	14	18 (n. b.)
7	Baked for 2 nights; 5 D	13	23 (n. b.)
8	Baked for 4 nights	7	18
9	Baked for 8 nights	15	21 (n. b.)
10	Baked for 4 nights; 3 D. Annealed straw; 7 D	23	25 (n. b.)
11	Baked for 8 nights. Annealed dark straw; 2 D	19	22 (n. b.)
12	Baked for 14 nights. (Straw to blue)	13	20 (n. b.)
13	Quenched red in cold water; 4 D. Baked one month; 28 D	20	33 (n. b.)
14	Quenched violet in cold water; 4 D. Baked one month; 28 D	6	11 (n. b.)

[1] See Table II and diagrams for tensile tests.

TABLE VIII.—*Bending-tests of Siemens-Martin steel, &c.*—Continued.

No.	Particulars.	Number of bends. Cracked.	Broke.
	Red and blue hot, quenched in cold water.		
15	Quenched red-hot; 4 D	11	19 (n. b.)
16	Quenched red-hot; 4 D	15	20
17	Quenched blue; 6 D	....	10 (n. b.)
18	Red-hot, quenched edge; 4 D	18	25 (n. b.)
19	Blue-hot, quenched edge; 2 D	14	19 (n. b.)
20	Violet-hot, quenched edge; 2 D	15	19 (n. b.)
	Broken while blue-hot.		
21	Blue; fracture straw	....	5
22	Blue	....	4
23	Violet	....	2½
24	Light straw; fracture very light straw	....	7
25	Quenched red in cold water; 4 D. Violet	....	4
	Flattened hot and cold under a steam-hammer.		
26	Reduced 12 per cent. cold; 2 D	15	21 (n. b.)
27	Reduced 18 per cent. hot (blue); 2 D	....	0
28	Reduced 12 per cent. hot (violet); 2 D	6	0
	BENT COLD AFTER PRELIMINARY TWISTING.		
	Four twists of 90° in a length of 6 inches.		
29	Quenched red in cold water; 4 D. Twisted cold	10	15 (n. b.)
30	Quenched violet in cold water; 4 D. Twisted cold	14	18 (n. b.)
31	Twisted hot (blue to straw); 2 D	....	1
	BENT COLD AFTER PRELIMINARY BENDING.		
	One preliminary hot bend.		
32	I hot bend (blue); 7 D	9	13 (n. b.)
33	I hot bend (dark straw); 7 D	9	14 (n. b.)
34	I hot bend (light straw); 7 D	3	6 (n. b.)
	Two preliminary hot bends.		
35	II hot bends (blue); 7 D	....	2
36	II hot bends (dark straw); 7 D	....	0
37	II hot bends (light straw); 7 D	....	0
	Several preliminary cold bends.		
38	II cold bends, annealed violet; 4 D	17	19 (n. b.)
39	II cold bends, quenched violet in cold water; 4 D	9	11 (n. b.)
40	Quenched red in cold water; 4 D. IV cold bends; 3 D	10	13 (n. b.)
41	VIII cold bends, annealed blue; 4 D	2	9 (n. b.)
42	VIII cold bends, quenched violet in cold water; 4 D	1	1½
43	VIII cold bends; 50 D	7	15
44	Annealed blue; 7 D. VIII cold bends; 50 D	....	0

DISCUSSION.

Sir FREDERICK BRAMWELL, President, in proposing a vote of thanks to Mr. Stromeyer, said that the paper dealt with a subject of the very highest importance. Year by year mild steel was being substituted for wrought iron, and no doubt with very beneficial effects, whether in regard to its employment for ship-building, bridge-building, boiler-making, or in the construction of artillery. Nevertheless there were occasional instances of failure which excited alarm, and he thought that the institution could not be better occupied for one or two evenings in considering with the author whether it was possible to discover what were the causes commonly regarded as obscure which had led to the few instances of failure that had been recorded.

Mr. STROMEYER wished, before the discussion commenced, to draw attention to the stress diagrams, especially Figs. 6 and 7, as the influence of various preliminary operations was well shown in them. Thus it would be seen that the unprepared and quenched and the cold bent and twisted pieces gave concave curves, while the others, which were either baked, bent, or twisted at a blue heat, gave curves which were almost straight. There was also a considerable difference shown in Fig. 4, the material being Lowmoor iron. Four specimens had been treated at a blue heat, and four cold. He could not give any reason for these differences; but he had published the tests, so that any persons making experiments might be able to compare the qualities. He also wished to refer to a tensile-test made with a piece of steel that had been drawn out blue-hot. Previously to the above experiments, he had stretched an ordinary test-piece after it had been flattened and thereby elongated at a blue heat, and the result was shown in Table I, No. 22. The steel from which this piece had been cut had been tested after annealing, and found to have (see No. 11) an elastic limit of 19 tons, and an ultimate breaking-stress of 29.7 tons, with an elongation of 20 per cent. In the flattened piece the limit rose to 20 tons, the breaking-stress to 32 tons, and the elongation was reduced nearly one-half, viz, 12.9 per cent. This showed that although the metal was made stronger by flattening it at a blue heat, its ductility was decidedly reduced. He exhibited a test-piece which had been prepared in order to show the effect of working steel at a blue heat. It was ½-inch thick, and sheared wide about 2 inches. One end was bent nearly double when cold. The other end was put between two pieces of red-hot iron, and when blue-hot was flattened under a steam-hammer to about two-thirds of its original thickness. After an interval of a day it broke with one heavy blow of a hammer, and the fracture, as would be seen, was perfectly crystalline. Another piece, but of smaller dimensions, had been prepared in the

same way by being drawn out at a blue heat at one end, while the other one had been bent double to show the ductility of the metal. He then broke this piece by striking it on an anvil, and pointed out that it had broken at a point where the heat had not exceeded a straw color. This peculiarity of steel might perhaps explain how bayonets, about which a good deal had recently been said, had been spoilt. It was possible that after they were drawn out at a red heat the finishing was done while they were blue-hot. Some friends had suggested that if the working of steel or even iron at blue heat spoiled it permanently, rivets of any sort would not be able to stand, for although they were red-hot to begin with they must pass through a blue heat before cold. It was a fact, however, that riveting did stand, and his explanation was that in all riveting, especially in piece-work, the riveters after hammering a rivet at red heat left off work for a time while engaged in hammering the next one, and sometimes two; when this was done they would go back and re-rivet (harden up) the first one. Thus they left off while the rivet was going through the temperature of the blue heat. It was red to begin with, and the first part of the hammering was done then; as it cooled to blue it was in a dangerous condition, and the hammering was stopped; then, when it became cold enough to be hammered again, the work was completed. He thought that if the rivets were worked continuously from a red heat till they were quite cold they would often break. He noticed in the case of ships that had been in collision, and where it was necessary to cut out many rivets, that three or four out of a dozen would always appear with a crystalline fracture. This, in his opinion, was due to the hammering having been continued too long or recommenced too soon. He did not suppose that the workmen understood the question of the blue heat, but they had evidently found out by practice that if they continued to hammer until the rivet was cold the result was unsatisfactory, and that they had to do it over again.

Mr. Percy C. Gilchrist observed that the members would be still more indebted to the author if he would kindly supplement the paper by analyses of the samples upon which he had operated. It was a satisfaction to know that, for the first time, iron and steel appeared to be in the same boat, both being injuriously affected by a blue heat. Reference was made in the paper to a steel plate which cracked while it was being straightened, due, he thought, to too much reliance being attached to the quench test, a test that was excellent for quickly ascertaining if the sample operated upon contained too much silicon, phosphorus, manganese, and carbon, but which supplied no information of the physical condition of the material operated upon. If a plate had been produced from an ingot or bloom, one end of which had been burnt and which had not received sufficient work to destroy the ill-effects so produced, or if the plate had been injured in the rolling, one end perhaps having been rolled too cold, the quench test would then show the plate to be ductile; and yet if a piece were tested

straight from the shears it would break without bending, the discrepancy between the two tests being due to the fact that, in quenching, the test-piece had been practically annealed, for making it red-hot annealed it, and the subsequent quenching in cold water did not affect the bending properties of good soft steel. In order, therefore, to rapidly tell whether the steel was right both chemically and physically, he suggested that pieces from the plate representing each end of the ingot should be tested, both after quenching and also direct from the shears. In this manner it would be shown, not only whether the steel was right chemically, but also whether it was left in its softest physical state. When the quench test stood better, as it often did, when the test came straight from the shears, it was not in its softest physical state, and should be annealed and retested, and if the tests did not then agree the plate should not be used. The spoiling referred to by the author when bending at a blue heat appeared to him to be analogous to that caused by stamping steel. Thus, suppose a piece of steel were punched into a bowl, and were then left until the following day, the bowl would not submit to be stamped deeper without working; that was, the steel had been made physically hard; if, however, it were annealed and brought into a soft state, say with all its particles at rest, then it could be stamped still deeper. Probably if the steel that had been subjected to a blue heat were afterwards annealed it also would be restored to its original ductility. As to the use of the word "mystery" in connection with failures in steel, he thought this was but a polite term to express our ignorance as to the best and proper way of treating steel. There was no doubt that, although mild steel surpassed the best iron in possessing equal strength lengthways and crossways, and also in possessing greater elongation and contraction of area than the best iron, yet that it required somewhat different treatment to iron. Nevertheless the lower the tensile strain possessed by steel the more nearly did it approach the best Yorkshire iron, and the more abuse it would stand, such as cold rolling, punching, shearing, heating it unequally, &c., without being spoilt. Thus, if a steel were taken containing carbon 0.05 per cent., manganese 0.3 per cent., phosphorus 0.05 per cent., with 23 or 24 tons tensile strain, it would also have 25 to 39 per cent. of elongation, 55 to 65 per cent. contraction of area, and the same strength longitudinally and transversely, and would be spoilt with difficulty. If engineers would keep to low tensile strains he felt sure that the material would be less easily spoilt than harder steels, and that there would be fewer so-called mysteries to be accounted for.

Mr. E. H. Carbutt, M. P., said that although he was an engineer and an associate of the Institution, he had not followed his profession for the last few years, and therefore his education was not up to the present day. He had come, however, to learn something about a question which had that day been discussed in the House of Commons—the question of defective bayonets. There had been a great outcry in the

country because the bayonets supplied to the army had failed, and during the Egyptian war especially had proved very defective. It had been proved, he believed satisfactorily, that one-third of the bayonets supplied to three regiments had proved defective. That was a very serious matter, because if one-third of the soldiers found their implements defective, they would lose confidence, and not exhibit the acts of gallantry to which Englishmen had been accustomed. He should be glad, therefore, if the president, who was a member of the ordnance committee, could tell the meeting something about the question of annealing steel, which perhaps, as the author had suggested, might have something to do with the grievance complained of. It appeared that some of the bayonets had been made from German steel, and some from steel supplied by one or two leading firms in Sheffield. The Government had promised that a report should be presented upon the question. He greatly disapproved of the way in which English arsenals were conducted; and he hoped a change would soon be effected in their management in a right direction. With reference to the annealing of steel, he might mention that on lately visiting Woolwich he noticed the process of shrinking the hoops on the guns. They were warmed by being put into a fire made of pieces of wood, in the open air. His own opinion was that the hoops ought to be shrunk on with a certain tension, and at a proper temperature. It seemed a barbarous method to heat them out of doors by pieces of wood at an unknown temperature, and then to shrink them on. Surely the largest manufacturing nation in the world ought to be able to do better than that.

Mr. Edward Reynolds thought that the peculiar behavior of material to which reference had been made was wholly a question of quality, and ought not to be accepted as indicating a normal and invariable condition. With regard to bayonets, it might be taken for granted that, whether or not in the course of manufacture they had been worked at a blue heat, they would certainly afterwards have been made red-hot, which would remove any special molecular condition arising therefrom; they would have been hardened in water, and then brought purposely to a blue heat, giving the spring temper, which was the most trustworthy condition known. Many of the tests reported showed that the material was soft. That again might be a question of quality of material, or of what steel makers called right temper, which meant carbonization, not what the scientific world called temper; but it was equally likely to have resulted from the notorious habit soldiers had of using their bayonets as pokers. &c. Another example of working at a blue heat occurred in file hardening, which he had often watched in file makers' works. In the ordinary way, when a file was hardened, it was brought to a uniform red-heat by being immersed in red-hot melted lead; it was then dipped in water, probably with some solution in it, and it was cooled to just about "blue heat" and taken out again, being so hard that the teeth were not easily damaged. If warped in hardening, as frequently hap-

pened, a man with a lever prized it under a bar, and then hammered it on a lead or wooden block with a heavy mallet. He then dipped it again, and the hot file made a decided fizzing in the water. If the simple fact of working at a blue heat was fatal, that would be impossible; and yet it was done thousands of times every day. He had seen such heavy blows given that he believed the file could not possibly stand them if it were cold. He did not make these statements in order to controvert facts which were indisputable. The authorities who brought such matters forward never did so lightly, but always with the greatest care. The conclusion which he wished to draw was, that far too much was expected from the material, especially in relation to its price. During the last twenty-five years he had had very little to do with what might be called inferior or common materials; but that day before leaving his works he thought he would try how the materials at hand would behave. He first took three pieces of Swedish bar-iron 1 inch in diameter; they were brought out of a store where they were exposed to frost. One piece was bent cold; another piece was ground bright and heated until it was deep orange, and then bent; a third piece was heated red-hot, as he thought the question might arise whether the heating up or the cooling down affected the question. All the three pieces were bent close with a steam-hammer, so that the double bar measured rather less than twice the original diameter. The first specimen of Swedish iron, after being heated and cooled, did start a pile seam, a little of it peeled up; he therefore took another, which bent close without any such result. The next was a piece of steel such as was used for rivets for girders, of about 28 tons to the inch tensile strength. The steel had been treated in a reeling-mill, which was a mill for rolling in a circular direction between rolls, so as to make the bar circular, it being intended for cold riveting. The steel here was always rolled until it was practically black. The result was that there was no apparent difference in behavior between the piece taken out of the store exposed to frost, after being finished at a supposed blue heat, and those specimens heated up and cooled down, except that the men thought that the piece heated up bent rather more easily. He repeated the experiment with some very mild steel, and the result was the same. Of course, if a number of flexures had been tried, it was possible that a difference might have been found, but no one ever practically wanted iron or steel to do more than bend double without any sign of distress. With regard to the particular heat in question, about forty years ago there were published some experiments by the late Mr. (afterwards Sir William) Fairbairn with reference to the supposed effect produced on the strength of the boiler-plates by the heat of the steam in the boiler. The plate had the highest tensile strength at about 600°; and although the experiments were made without analyses, it was singular that the heat at which the iron was the strongest, and to which steel had to be raised in tempering to bring it to the spring temper, was this blue heat; it was certainly very strange if it was necessarily dangerous in other things.

Mr. H. H. West doubted whether the blue heat was the critical condition that it was generally supposed to be. He did not know that he could give any good reason for his opinion, but possibly the conditions which were really critical were masked under the blue heat, something happening of which engineers were not cognizant. He had thought that there might be a sort of recrystallization, and that it occurred at or about the heat in question, when the ultimate molecules of the steel were in a state of want of cohesion among themselves. If a little fusible metal was allowed to cool until it was nearly solid, it became exceedingly friable, and broke up into almost a powder, and came into a sort of pasty granular condition. Possibly a somewhat analogous condition occurred in steel at or about that temperature, and if in that state it received any violence, a disturbance of the cohesion of the particles might not reach the surface and be palpable to the eye or to any physical test, and yet, as soon as certain circumstances arose, the material might be fractured in an alarming way. No doubt many members had heard of the extraordinary accident that happened to a set of marine boilers, an account of which had appeared in "The Engineer."[1] In that case he had been much struck with the want of consistency between the tests made of the material after the accident and those that had been made before. One thing had especially attracted his attention, that whereas the average tensile strength of the material by the tests at the works was from 27 to 28 tons per square inch, in no case exceeding 30 or 30.3 tons, and in no case sinking below 26.5 tons, when it was tested after the accident it was found that some of the steel had sunk as low as 24 tons; and another plate, which apparently belonged to the same quantity of steel, and presumably had gone through the same tests, had risen to 40 or 47 tons. Assuming that the steel had altered its condition between leaving the works and the time of the accident, it was hardly likely that the same conditions would degrade the tensile strength per square inch from 27 or 28 tons to 24 tons, and also enhance it to 40 and 47 tons. That discrepancy seemed to suggest that the whole of the steel had not been thoroughly tested, and that consequently testing had been blamed for a defect that was really personal, the test not having been carried far enough. Some years ago, when mild steel was first brought into use for ship-building purposes, a case happened that gave him a great deal of concern. Some of the angle-bars for forming deck-beams were being straightened and set in the squeezers previous to being put into place. The rivet-holes for the deck-plating came to about the middle of the flange of the bar. On being set in the squeezers, bar after bar flew out from the hole to the edge. That was rather alarming, because it was very short, and indicated that the metal was not trustworthy. Further experiments with some other bars showed that the holes, instead of flying out suddenly and sharply to the edge, elongated, and the material on the tension side of

[1] December 11, 1885.

the hole became locally contracted, and was pulled down like a piece of toffee. That suggested to his mind the desirability of instituting a test which he had called the edge-bending test, and which he had frequently adopted with satisfactory results. The method was to take a piece of plate or bar, and punch or drill a hole in it, and then apply the edge-bending test. If it flew suddenly from the hole to the edge of the bar or plate, he considered that the material was hard, or defective in some way, that would justify him in condemning it. If, on the other hand, it elongated, as in the case he had spoken of, he thought the material was trustworthy. He had never known that test to fail. With reference to the testing of steel, his impression was that if it was carried out systematically and thoroughly, in a way in which he believed Lloyd's were now carrying it out, practically examining every plate, the testing would be sufficient to show the condition of the material. The question of blue heat required a much larger number of experiments before engineers could satisfy themselves as to the points raised by the author.

Mr. W. PARKER said that it had been well known for a long time to the authorities at Lloyd's and the Admiralty that steel at a blue or black heat was brittle; but it had never occurred to him, nor, he believed, to the Admiralty authorities, that the brittleness was brought about by putting work on the steel when at that temperature. That, however, opened up a very grave question, for it had been the practice of numbers of engineers to work steel at those heats, not only in flanging but also in bending mild plates. When mild steel was introduced for boiler-making and ship-building purposes, in 1878, it was thought that it did not require any different treatment from iron. It had been often stated that this material could be knocked and hammered about as much as iron with impunity, but he thought that every one who had had experience in working steel knew that it could not be so treated. It was, as had been stated, a different material from iron, and should be worked in a different manner. One of its peculiarities, as the author had pointed out, was that it became extremely brittle when worked at these dangerous heats. The late Sir William Siemens had endeavored on many occasions to show why peculiar fractures took place in steel plates. He represented steel as being of a homogeneous, and iron of a heterogeneous nature, iron having fiber in it and steel none; and he went on to say if strains were set up by local heating or by shearing or punching, and they were more than sufficient to overcome the strength of the plate at any particular point, a fracture or tear would certainly take place, which many persons might regard as "mysterious." It had been Mr. Parker's privilege since 1878 to investigate nearly all the accidents of importance that had occurred to steel plates in this country, and he would briefly explain a few typical cases. The first of any importance was on the Tyne. Two steel boilers were being constructed for the mercantile marine; one of them was made satisfactorily, but the other met with an accident. The boiler was about 10 feet in diameter, and built to work at a pressure

of 70 pounds to the square inch. It contained three furnaces, which were riveted to the tube-plates and to the front-plates by a single row of rivets; the boiler was almost completely riveted, in fact, the men were just completing the riveting of the front edge of the furnace when they left the boiler to go to dinner; on their return an hour afterwards, to their great surprise the furnace-plate had torn for a distance of about 18 inches. Every one then said that the plate must be a bad one, but on analysis it was found that the material was as good as could be made, having come from the best makers in the country at that time, namely, the Landore Steel Company and been made by the Siemens-Martin process. The tenacity of this plate was not more than 28 tons to the square inch, and it had an elongation of over 26 per cent. in a length of 8 inches. By accident the bolts that had been holding the furnace up to the mouth-piece or front-plate of the boiler had not been taken out. When they were taken out it was discovered that the rivet-holes, although originally drilled through both plates in place, were very unfair and quite one-eighth of an inch blind, proving that the furnace itself had originally been much less than the circular part of the front plate to which it had to be riveted. The men in riveting stretched the furnace-plate and made it accommodate itself to the size of the front plate, and threw strains upon the material sufficient to tear it when the proper time arrived. Had this not been found out the plate would have been considered to be bad steel. It had been said that when an accident occurred to a steel plate a great deal of noise was made about it, while nothing of the sort took place with regard to iron. For his own part, he thought that whenever engineers met with any "mystery," it was their bounden duty to try and get to the bottom of it in the interests both of steel makers and steel users. He had therefore taken great personal interest in all such cases that had come before him. The next case was that of a set of boilers, which were made for the Emperor of Russia's yacht "Livadia," by Messrs. John Elder & Co., of Glasgow. This vessel was to be fitted with eight boilers, to work at a pressure of 90 pounds per square inch; the shells were 14 feet 6 inches in diameter, and three-fourths of an inch thick; the steel was obtained from one of the best makers in the kingdom. The boilers were not under the personal inspection of Lloyd's surveyors, but the steel was tested by the makers in the usual manner, and also by Messrs. John Elder & Co., and pronounced to be quite satisfactory. When water-pressure was being applied to the first boiler, before it reached anything like twice the working pressure, the shell of the boiler tore across the plates in three places, and on examining another of the boilers that had not been tested it was found to be cracked in five or six places without having had any pressure upon it at all. The shells were taken off the whole of the eight boilers and new shells made, but even at that time, and in face of the apparently unaccountable accident, so much confidence was felt in steel that iron shells were not substituted, steel being ordered from works on which more reliance could be placed. He had

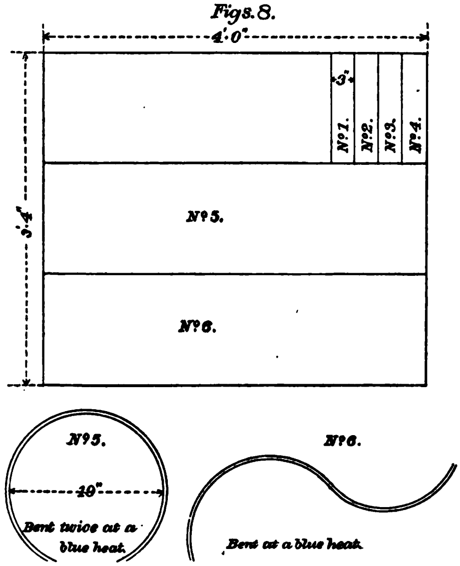
been requested by the committee of Lloyd's Register to try to get to the bottom of the "mystery." He went to Glasgow and had a piece of the plate sent to London, where he found it in its then condition to be so brittle that it could be broken with an ordinary hammer; but when it was annealed and the strains that existed in it thus set at rest, it was perfectly ductile and bent double without any signs of fracture. The tenacity of the plates was everything that could be desired, and so were the elongation, ductility, and chemical analysis. It was ascertained that after the plates were rolled they were piled one on the top of another in an annealing furnace, and sent from there to the shipyard at Glasgow. It occurred to him that the annealing done in this manner might have set up some strains of a serious nature in the plates, but nothing could be stated definitely on this point. An experiment was made with a view to ascertaining whether the plates had had sufficient work put upon them. A piece of the plate was rolled down to half its original thickness, and when tested it was found that after the extra work had been put into the material it could be bent or doubled, or anything done with it. This result seemed to point to the conclusion that the plates had not had sufficient work put upon them, but he was never perfectly satisfied with that conclusion. He was, however, satisfied that the steel in its original state was, unlike the material in the other case referred to, bad steel, and quite unfit for the purpose for which it was intended. The next case to which he would refer was that of a boiler made in the beginning of last year at the Wallsend Slipway Company's works on the Tyne. It was about 14 feet in diameter, and constructed to work at a pressure of 150 pounds per square inch. The plates that gave way were shell-plates, and stood all the mechanical tests that were required. They were 20 feet long, 5 feet 6 inches in width, and 1¼ inches thick. When the boiler was being tested the shell burst. The pressure was being raised to 300 pounds per square inch, and as it reached 240 pounds the plate tore through from end to end. In endeavoring to get to the bottom of this occurrence he had pieces cut from the plate and tested, and they quite corresponded with the tests which Lloyd's surveyors had carried out at the steel works on specimens of the material; the analysis of the plate was found all right, except that the amount of carbon was twice as high as in any other plate he had ever seen to stand the same mechanical test. This was so striking that he looked further into the matter. He visited the steel works, and the makers assured him that a plate of 30 tons to the square inch, and 1¼ inches in thickness, must necessarily be much higher in carbon, because the mechanical work put upon it was not nearly so much as in the case of a plate ½ inch thick. They told him that every thick plate thus required more carbon in it than a thin plate intended to stand the same mechanical tests. The makers took him round their works, and, pointing out different ingots, said, "These ingots are for ½-inch plates intended to have a tensile-strain of 30 tons to the square inch; those are intended for plates an inch thick, intended to stand a strain of 30 tons;

and those are intended for plates $1\frac{1}{4}$ inches thick to stand 30 tons; and the carbon, which is the tempering property, varies in every one of them." He read a paper before the Institution of Naval Architects on this subject, in which he endeavored to show that with the enormous plates now used in high-pressure boilers, engineers were, perhaps, unknowingly drifting into a somewhat dangerous material;[1] and from that day to the present Lloyd's Register Society had endeavored to discourage rather than encourage large thick plates of high tenacity. The next case, which differed from all the preceding ones as far as the investigation went, took place only a fortnight ago. It was that of a boiler which was being made at Hull, about 10 feet in diameter, to work at only 90 pounds pressure per square inch. It was riveted and complete, and while being tested the shell-plate failed, tearing through from end to end, much in the same way as in the last case, one part bulging considerably, and the other remaining in its normal position. This plate was only $\frac{11}{16}$ inch thick, and had never been heated at all. So far as he could ascertain, the analysis was everything that could be desired as were also the tensile and bending tests; still the plate cracked when only one-third of its tested strength had been reached. That brought to his mind a remark made by Mr. Gilchrist, that it was not uncommon to roll plates when one end was colder than another, or to roll them while cold, and deliver them to the users with initial strains existing in them almost sufficient to tear the plates without their being worked. In such a case the test-piece might give a satisfactory result and yet convey no idea of the real state of the plate itself. This of course could be avoided if every plate were properly annealed after having been rolled and sheared. Mr. West had referred to the boilers in the "British Prince" and "British Princess," which had caused so much commotion. The material in that case, and the boilers themselves, were made four or five years ago, so that it was ancient history so far as Mr. Parker was concerned. The material was tested by Lloyd's surveyors, and it was well known that in this particular case they had to condemn a great deal more material than they passed. Their tests were not at that time of such a nature that they could test every plate. Figs. 8, Nos. 5 and 6, represented the method adopted of bending a plate for testing purposes. The works where the plates were made had small Bessemer converters, the first, he thought, that Sir Henry Bessemer had made, and were only capable of turning out a charge of some 30 cwt. each. The plant, rolling mills, &c., were of the lightest description; the hammers were not heavy enough to put a sufficient amount of work on the material, and very thin and very small plates were habitually made. In this state these works branched out into the marine-boiler trade, and it could not be wondered at if the manufacturers got into trouble at first. It was their practice to

[1] Transactions of the Institution of Naval Architects, vol. xxvi, p. 253.

cast the ingots flat, in the form of a slab, about 5 inches thick, instead of casting them in the form of an ingot of about 20 by 20 inches thick; these slabs they heated and passed through the mill, and produced plates from them. The plates in question were made at those works, and though they were tested, they seemed by some means to have escaped the notice of those who were responsible for the quality of the material. The makers, seeing that their existing plant was not powerful enough to make a satisfactory material for marine boilers, had since put down Siemens furnaces, had erected large hammers, and now turned out of their mills as good steel as any firm in the kingdom. With reference to the remarks of the president as to the increasing use of steel, he might be permitted to say that when the first mild steel marine boiler

Figs. 8.



Method of bending a plate.

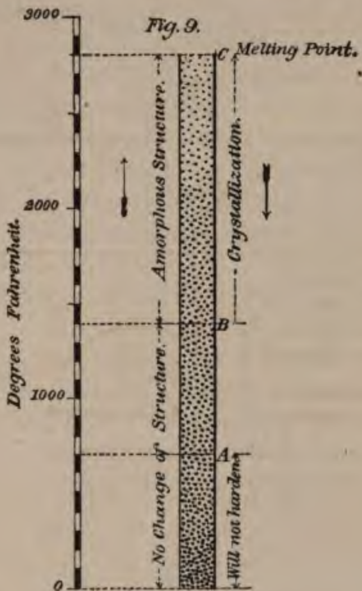
was being made in 1878, he had the honor to read a paper before the Institution of Naval Architects, the concluding remarks of which, if it were not considered egotistical on his part, he should be glad to read:

Now, as we have a material that gives us a boiler about 30 per cent. stronger than an iron boiler of the same scantlings, and as it seems possible that we may be able in the immediate future to dispense entirely with longitudinal riveted seams, by having the shells rolled, and as there has also been a furnace introduced which can be worked at twice the pressure of the ordinary plain flue, it does appear to me that we have succeeded, in a great measure, in removing the old conditions that have militated against much higher pressures being obtained, and that we appear to be now in a position to make a fresh departure in the direction of still greater pressures. If the improvements which I have indicated prove, as I have little doubt they will prove, successful, we shall have gained an advantage represented in the aggregate by an increase of about 80 or 90 per cent. of the working pressure. In other words, we shall be able to work the present form of boiler at 160 or 170 pounds per square inch;

and although the resultant economy will not be so great as that which attended the increase at one step from 30 to 60 pounds, we may confidently anticipate that it will be sufficient to give a great impetus to steam navigation, advancement in which has lately been so much retarded by the high consumption of fuel.[1]

He was glad to say that, with the exception of rolling shell-plates solid, all this had been attained; and up to the present time there had been made over 4,000 marine boilers, representing over 160,000 tons of steel, and there had not been one accident under steam. Besides that, the advancing pressures had brought to the front an engine—the triple expansive engine—which had conferred upon the ship owners of this country a boon represented by quite 20 per cent. less coal than was burned before the introduction of steel. He believed he might safely say that the introduction of this material had been the means of an improvement in marine engineering such as had not been witnessed during the last twenty years.

Mr. W. ANDERSON said that the author of the communication under discussion did not appear to be aware that in 1868 Mr. Chernoff read a



Chernoff's theory, 1868.

paper on the effect of temperature on steel, at a meeting of the Imperial Technical Society of St. Petersburg. Mr. Anderson translated that paper in 1876 under the auspices of the institution, and the steel committee of the Institution of Mechanical Engineers republished the translation.[2] Mr. Chernoff, as forge-master of the Abouchoff Steel Works, had unrivaled opportunities of studying the behavior of steel in forg-

[1] Transactions of the Institution of Naval Architects, vol. xix, p. 181.
[2] Proceedings, 1880, p. 225.

ings of from 40 tons weight downwards. The theory which he propounded was this: he divided the whole range of temperature from 32° to the melting-point of steel into three zones (Fig. 9). Steel confined to the temperatures in the lowest zones, the limit of which he called A degrees, could not be hardened, no matter how energetically cooled; steel raised to any temperature in the middle zone, the superior limit of which was at B degrees, did not undergo any molecular change, though some qualities would harden on being suddenly cooled; and, finally, steel heated above the temperature B adopted an amorphous or wax-like structure, and became very plastic, the said plasticity extending up to the melting-point. He next drew an analogy between the behavior of a hot concentrated solution of alum, and steel in the highest zone. It was well known that a hot solution of alum, if allowed to cool slowly, would solidify in large crystals; if allowed to cool slowly, but if kept agitated, it would solidify into small crystals; if made to cool rapidly it would also form a finely crystallized solid; and finally, if made to cool rapidly, and if kept agitated as well, the finest crystals would be formed; and so it was with steel. If heated above the point B and allowed to cool slowly the metal would become coarsely crystalline and unfit for use. If cooled slowly, but well worked all the time, a fine silky grain would be obtained, and this result would be still more apparent if the cooling took place moderately quickly. If cooled suddenly, as in tempering the large masses of metal forming guns, a fine and uniform grain would result. The temperatures A and B varied with the chemical composition of the steel. In pure steel, a combination of iron and carbon only, Mr. Chernoff stated that the temperatures became lower in proportion as the percentage of carbon increased. He had prepared some samples of boiler-plate for the purpose of illustrating Mr. Chernoff's views. He had four specimens cut from the same lot of plates, and annealed them all by heating them above the temperature B and then allowing them to cool quickly on an earthen floor. The samples were, by this means, all brought to the same molecular condition. No. 1 was doubled up flat under a steam-hammer cold, and then broken open. Not the slightest crack was observable on the outside of the bend. No. 2 was heated to a blue heat and doubled up under a steam hammer, then broken open, and no crack whatever could be detected. No. 4 was kept at a bright-red heat in a hollow fire for ten minutes, then lightly but rapidly forged by two hammers on a cold anvil till the temperature had fallen to a blue heat; it was then doubled up under the hammer and broken by opening out. Not the slightest crack could be detected. Comparing the fractures of the three samples, no difference could be detected. No. 3 was heated to a bright-red heat in a plate-furnace last Saturday, and was allowed to cool very slowly with the furnace. The specimen was then doubled up cold and broken open like the others. No distinct crack appeared on the outer surface of the bend, but the strain had revealed the boundaries of a

crystallized structure by a network of incipient cracks, and on the inside two cracks had started besides the one at which the bar broke. The fracture was much more coarsely crystalline than that of the other specimens. This showed that the very perfect annealing received was a positive injury, a circumstance which Mr. Chernoff's theory would suggest. Mr. Kirkaldy had noticed this effect, for he stated that iron was injured by being brought to a white or welding heat if not, at the same time, hammered or rolled. Now it was evident that had the author got hold of sample No. 3 he would have found it brittle at a blue heat, whereas if he had heated it first above B and then cooled it quickly, a contrary result would have been obtained; consequently to have made his observations valuable, he should first have annealed all his specimens in the proper manner, so as to bring them all to the same molecular condition. In rolling boiler-plates it was manifest that if the plates left the rolls at temperatures higher than B, and were allowed to cool slowly in heaps, those parts of the plates which were slowest in cooling would crystallize in coarse crystals, while the parts which cooled more rapidly would assume a finer grain, and internal stresses would consequently be set up, which would tend to make the plates crack spontaneously sooner or later, and this effect would be more pronounced with thick plates than with thin ones. He was convinced that it was from ignorance of this point that the apparent quality of steel plates was so uncertain. Iron was not so sensitive because the temperature B was very high, and iron plates always left the rolls well below it before being allowed to cool. He was at present engaged in condensing, for the minutes of proceedings, an account of some experiments on steel and iron girders translated from the proceedings of the Dutch Institution of Civil Engineers, by Mr. Siccama, M. Inst. C. E., and he found there a record of two steel beams the plates and angles of which were annealed before the beams were put together. To the surprise of the experimenters the beams behaved worse than other girders of soft steel, hard steel, or iron. It was not stated how the annealing was done, but he presumed in the same way as a whole beam was annealed, namely, by putting the plates into a furnace, heating them up to a high temperature, and then allowing them to cool slowly with the furnace. If the temperature attained was higher than B, Mr. Chernoff's theory explained cause of the failure, and the fact that samples cut from the plates showed high tensional resistance would be quite compatible with the observed results. He believed that it would be in vain to look for greater certainty in the behavior of steel until more attention was paid to the points raised so intelligently by Mr. Chernoff, although, undoubtedly, many other considerations depending upon changes in chemical composition or structure, due to alterations of temperature, which had been so carefully investigated by Sir F. Abel, should not be lost sight of.[1]

[1] Institution of Mechanical Engineers. Proceedings, 1881, p. 696; 1-83, p. 56.

Prof. W. C. UNWIN said that in spite of Mr. Gilchrist's protest he was afraid that steel behaved sometimes in a mysterious way, in a way which could not be adequately explained, and he thought it was of the first importance to get to the bottom of the explanation of the so-called failures, because until then it could not be known whether the empirical tests by which its quality was judged were the best tests that could be used. There was, he thought, sometimes a tendency to accept explanations as adequate that did not completely solve the difficulty. He might be permitted to refer to a case mentioned by Mr. Parker to show what he meant. It was a case of a boiler-flue which cracked, the steel being, as Mr. Parker believed, perfectly good. The flue was being riveted up to the end plate of the boiler; the circle of the rivets was partly completed; the men went to dinner, and while they were away the plate cracked. It was afterwards found, when the plate was taken out, that the rivet-holes had become foul about $\frac{1}{8}$ inch, and Mr. Parker put that forward as an explanation of the accident. On that two questions arose: first, why the plate did not crack when the stress came upon it, why it waited till the men had gone away? and, secondly, which was a more important question, how it was that in a material which would elongate 30 per cent. before fracture, the amount of strain indicated by the holes becoming foul to the extent of $\frac{1}{8}$ inch produced a fracture? The flue was 9 feet in circumference, and with 30 per cent. of elongation ought to have lengthened 3 feet before cracking. Again, looking at the enormous strength of the boiler-plate, it was difficult to see how the workmen could put strain enough on it to crack it if it were perfectly homogeneous. With reference to the paper, he would pass over the experiments on tension, because the author had drawn no definite conclusions from them. Ho would simply observe, with respect to the diagrams of strain and stress, that some experiments by Mr. Martens, in the laboratory at Berlin,[1] seemed to show that when the elongation was obtained in that way the curves were liable to errors + or − 5 per cent.; that would almost explain the irregularity of the curves. There were two series of results in the tables which were very important in considering fractures of steel. First, there were cases of plates which were bent while at a blue heat or a black heat, and which broke with a very small amount of bending. It had been known for a long time that there was a temperature below the temperature at which steel was plastic at which it was rather dangerous to work it. The board of trade, in 1881, had published memoranda giving a series of experiments carried out at the Steel Company of Scotland's works. Forty-eight plates were taken, and a strip was cut from each. Half of the strips were bent cold to an angle of 180° round a bar of a diameter equal to twice their thickness. The whole of the plates bent cold stood the test. Corresponding strips were bent at the temperature of boiling tallow,

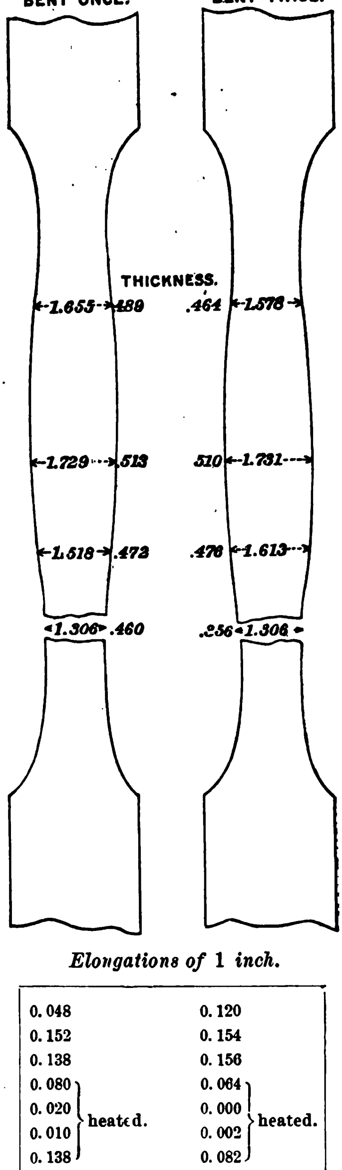
[1] Ueber die Bestimmung der Zähigkeit der Materialien. Mitt. aus den K. Tech. Versuchsanstalten zu Berlin, 1884, p. 93.

and every one of the strips bent hot cracked long before 180° was reached. But he did not think that these results threw much light on the mysterious behavior of steel, because engineers had no right to assume that either the physical or the chemical condition of the steel was the same when hot as when it was cold. The most striking results, no doubt, were those which showed that a plate which had been bent hot—at a blue heat—and then allowed to cool was very brittle after it was cold. Those results were no doubt much newer. The author explained them by saying that the plates had lost their ductility, and he supposed that must have been so. But there arose the question, why was it not possible to detect loss of ductility in the tension-tests? The results were of course extremely interesting, and he had no doubt that qualitatively they indicated exactly the conclusion that the author drew—that the steel was dangerous after it had been treated in that way. But the failures in steel which were most interesting did not occur while the plates were being bent, but while the steel was being subjected to the ordinary simple conditions of stress, and there was, therefore, a gap in those experiments in regard to the explanation of the failures of steel. He had lately tried to ascertain whether the tension-tests would not afford information as to the condition of steel after it had been treated in that way. He had taken a plate of 27-ton steel and broken it in the ordinary way. The longer half he formed into a new test-piece, and he treated the part to be tested to a temperature a little below redness, and quietly bent it in the testing-machine, and also straightened it in the testing-machine. It was not bent to any sharp angle, but only to a comparatively shallow curve without hammering or vibration. The plate was then tested by tension. In the first testing it stood 27 tons to an inch, in the second it stood 32 tons; in the first testing the contraction of area was 51 per cent., and in the second it had gone down to 21 per cent. He then proceeded in another way. He took some test-pieces of the same mild steel, which were heated in a forge to a good red heat. He then placed them in the testing-machine between an ordinary **V** block and a large round piece of iron, and quietly bent them to an angle of about 15° on each side from their original straight position The pressure was put on when the redness disappeared. They were straightened in the testing-machine in the same way, and then tested as ordinary test-pieces. Figs. 10 represented the result obtained. In the center of the curvature, where the bar was bent, there was hardly any measurable contraction of area, and hardly any measurable extension. On each side of the middle of the bar it was drawn away in the form shown, but somewhat exaggerated. The figures showed the widths and thicknesses of the bar. It would be seen that the bar had contracted everywhere except just in the middle. Over the heated part the elongations, it would be seen, were very small, and towards each end they were much larger. This proved that that treatment of the steel rendered it unhomogeneous, and it could be easily understood how

by treating a plate of steel in that way it might be rendered very untrustworthy. Suppose the rigid part of the metal, for example, to be

Figs. 10

BENT ONCE. BENT TWICE.



Elongations of 1 inch.

0. 048		0. 120	
0. 152		0. 154	
0. 138		0. 156	
0. 080	heated.	0. 064	heated.
0. 020		0. 000	
0. 010		0. 002	
0. 138		0. 082	
0. 287		0. 174	
0. 622	(break).	0. 610	(break).
0. 160		0. 232	
0. 100			

near the edge of a plate, and the strain in the direction of its length, then the rigid part of the plate would have to carry nearly the whole

load on the plate, and an unhomogeneous plate of that kind would no doubt crack under a load which apparently did not exceed the working tension of the steel. He was not putting that forward as anything like a complete explanation of all that occurred, but he thought it showed one way, at all events, in which the unhomogeneity of the plate might arise, and in which a dangerous condition of the plate might possibly exist. He had tried another experiment with steel, which hitherto had not been successful—making cuts with a diamond to ascertain if he could reduce the plate's strength. At present the plates generally were not affected by the cuts, and the strength of the bar was not much altered. It had been often stated that failures in steel were due to initial stresses in the metal. Mention had been made of the way in which castings of steel or of pieces of glass would crack under apparently no load, in consequence of the very large stresses existing in the material itself, owing to the way in which it had been cast. It had always seemed to him to be exceedingly difficult to imagine that in a tough material like steel there should be initial stresses capable of tearing a bar to pieces. A material which could extend like steel, would, he imagined, relieve itself of any conceivable initial internal stresses without cracking; but if the steel itself had different extensibilities in different parts, then the excessive stress in certain parts was due to the loading. It was not the initial stress in the material, but the want of uniform distribution which led to failure.

Mr. Thomas W. Traill said that according to his own experience the working of steel at a blue heat was a very imprudent thing, but there was something still more dangerous, and that was, after putting work on a hot plate to place it in the structure before it was efficiently annealed. He believed that nine hundred and ninety-nine failures out of one thousand had taken place with steel of the mildest description, and having a good elongation and a low tensile strength. There had been comparatively few failures with steel for shell-plates, but they had taken place in nearly every instance with plates that had had work put upon them. In the case of the "Livadia," mechanical tests had shown that the steel shell-plates had a mean tensile stress of about 26 tons, and an elongation in 10 inches of about 27 per cent. Unfortunately those boilers failed under the hydraulic test at a strain of about 8 tons per square inch net section. A joint cut out of the boiler gave way at a stress of about 9 tons per square inch net section. Two joints, constructed out of the plate in a similar manner to that in which they had been originally constructed in the boiler, gave way at about 11 tons per square inch net section. But there were other joints constructed after the plates had been annealed, and they did not break until the stress reached about double that of the joints that had been made with unannealed plates, but these plates were probably affected by the punch more than some others. There were still some engineers who were afraid of using steel; but he could safely say that, considering the

number of iron marine boilers that had been constructed since mild steel had come to be used during the last ten years, the percentage of failures in iron boilers had been considerably above those in steel boilers—a fact that ought to be consoling to those who were in doubt with reference to the adoption of the useful and trustworthy material, steel. Steel, like other metals, required peculiar treatment; and if manufacturers and boiler-makers would not give steel the kindly treatment that its peculiarities demanded they must expect to have a few failures. Professor Unwin had alluded to some experiments that had been published in 1881. Mr. Traill conducted those experiments himself seven years ago, and he could corroborate what Professor Unwin had said about the failure of the specimens heated to the temperature of boiling tallow. The reason why he had adopted that mode of heating was to insure that all the specimens should be of a uniform temperature, and in bending them at that temperature they failed. In writing the report conjointly with his assistants, he had remarked that no branch of experimental research was more worthy of attention than the properties of mild steel at various temperatures up to 1,000°; but he was sorry to say that no one had yet come forward who would spend the money required, for it was a costly thing to conduct a series of trustworthy experiments to prove exactly at what temperature steel became dangerous. He had almost hoped that he had persuaded a steel-maker to have a series of such experiments made; but the cost had frightened him, and until some energetic steel-maker came forward for the purpose engineers would still be doubtful as to the employment of steel in structures, when its temperature was liable to become moderately high. If, however, they would only watch the workmen, they need not be afraid, because there had been no case of steel failing that could not be traced to improper treatment after it had left the hands of the manufacturers. The failures had not been due to the manufacturer, except perhaps in some solitary case of sending out a bad plate. It certainly was not to the interest of the manufacturer to do so, and the tests prevented it. As to the chemical tests he was afraid that they would be too troublesome and expensive for practical engineers to adopt. If mechanical tests gave good results, showing a fair amount of ductility, not less than 20 per cent. in a length of 10 inches, although he should like to see about 5 per cent. more, he thought there need be no fear of using that material. One failure, however, had taken place that had not yet been investigated, and he did not know any one who was able to state exactly the cause of it; but possibly that steel had not been made by a process like that employed for steel hitherto used in the construction of marine-boilers. In using steel for the shells of boilers, engineers knew very well that the plate was subject to little or no work, and those who adopted the practice of heating the plates before they bent them must expect to have failures. But there were engineers who rolled the plates cold up to 1¼ inches thick, and they said, "If the steel-maker cannot

supply us with a plate that will stand rolling without injury, we will not have his plates." He advised all who constructed boilers to prohibit heating when rolling plates for the shells.

Mr. W. H. Barlow, past-president, asked, if Mr. Traill referred to the process of annealing, to what temperature the material was raised, and in what manner it was cooled?

Mr. T. W. Traill said he was not speaking of experiments in annealing, except in the case of the joints he had referred to. The material was heated to a bright red, and allowed to cool gradually.

Sir Frederick Abel said he was not quite sure that chemists had made themselves sufficiently heard to convince the members of the institution that the chemical investigation of the properties of steel was of the importance that they, the chemists, attributed to it. The author had stated that his earlier experiments were made at a time when the peculiar influence of a blue heat was less understood than it was now. That had led Sir Frederick Abel to look through the paper carefully to see how the influence of the blue heat upon the steel was explained, and he confessed that he was there at fault. It had also led him to consider whether any knowledge that he might have acquired, in the laborious examination in which he had taken some part in connection with an inquiry into the condition in which carbon existed in steel, might contribute to an explanation of the peculiar behavior of steel pointed out in the paper. The interesting observations of Chernoff, to which Mr. Anderson had directed attention, he believed went far to explain such results as had been observed by the author. At the same time, he thought that certain results which he, together with his assistant, Mr. Deering, had arrived at, in their examination of samples of steel, might possibly have some bearing upon the question, and he would therefore throw out a few suggestions on the subject for the consideration of the members. In the course of the investigation it was found, in the first instance, that in annealed steel the carbon existed almost entirely in the form of an iron carbide of very definite composition, uniformly distributed in an extremely fine condition throughout the mass of the metal; and that, on the other hand, in hardened steel, it appeared as if the passing of the steel into the hardened condition did not allow time for the separation or elimination of the carbon in that particular form; that, in fact, the carbon existed in steel in the hardened form almost entirely as it existed in solution in the liquid metal. Very small proportions of the definite carbide were found in the hardened metal, but they were probably the result of some small amount of annealing which even hardened metal underwent in the course of cooling. The results which they found in examining steel which had been submitted to degrees of heat included under the general term "blue heat," were intermediate between those exhibited by the annealed metal and by the hardened metal. He had with him two specimens, which might serve to illustrate what he meant as to the condition of the carbon existing in the steel. In the vessel

which he exhibited there was a small platinum sieve (glass sieves were also used with fine perforations), upon which the little steel disk operated upon rested, in a liquid by which the iron was very slowly dissolved. As the iron dissolved the carbide of iron passed through the meshes of the sieve or the minute perforations in the glass, and collected at the bottom as a powder of a fine and uniform state of division. In the specimen exhibited it was fixed by means of gelatine, and it showed that the carbon was disseminated throughout the solution in an almost impalpable state of division. He had also a sample showing the result of a corresponding treatment of a portion of the same steel, blue-tempered, and in that case instead of the carbon passing through the sieve in a state of almost impalpable powder, or in a very finely granulated form, the portion which existed in the specimen as carbide of iron remained upon the sieve in a comparatively coarse condition, and had not been uniformly distributed throughout the original mass. It presented, therefore, in regard to the condition in which it existed in the metal, a decided difference as compared with the carbide from the other annealed sample. It was evident that the difference in the condition of the carbon existing in annealed steel and in the tempered steel might exert an important influence upon the strength or the behavior of the metal, for if the carbide was not uniformly disseminated throughout the steel, but irregularly distributed in comparatively coarse portions, it might seriously affect the working properties of a specimen which had been subjected to a particular heat favorable to the partial separation of the carbide. It had occurred to him, on looking at the results obtained in the course of their investigations, that these circumstances might possibly contribute towards explaining the causes of the behavior of steel when submitted to the action of blue heat. For example, whereas in the case of annealed steel they had separated from particular descriptions of steel from 12 to 13 per cent. of carbide of iron, containing almost the whole of the carbon in the steel, while in hardened steel there was always much less than 1 per cent. thus separated, in the case of steel of a straw temper there were separated 7½, 4½, and 5 per cent. of the carbide, and in the case of blue tempered steel between 4 and 5 per cent.; so that the proportions were intermediate between those found in the hardened and in the annealed metal. He did not ask the members to lay very great stress upon those facts, but they might possibly indicate one of the conditions affecting the behavior of the metal when subjected to blue heat. He had felt some regret in noticing that it was rather difficult to compare the results produced in the different experiments by the author, because the difficulty (referred to by a previous speaker as a formidable one) of submitting the samples to chemical examination had not been faced. No true history had been given of the specimens of steel; they were only distinguished as coming from different makers, and as being mild or harder steel; so that it was very difficult for a chemist to draw any inference from his point of view as to what might be the causes of

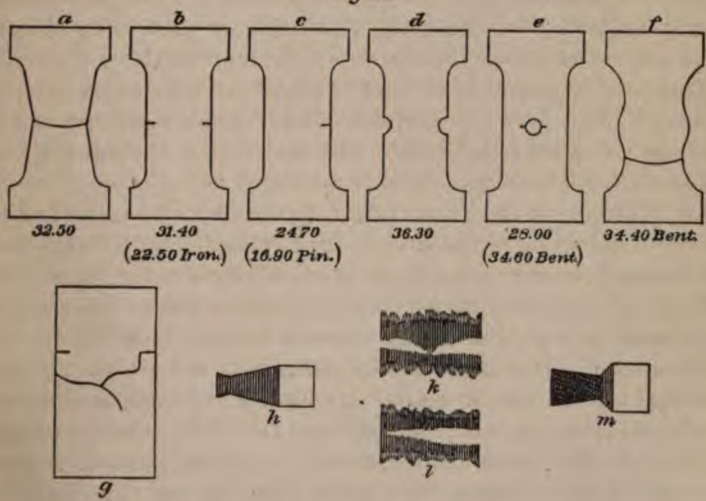
the differences observed. The author had spoken of the effect of baking in a core-molder's oven some samples which he had exposed to the different tests. It was somewhat difficult to reconcile the different results, and to offer anything like a satisfactory explanation. Yet even there he found that the results obtained in his examination of samples of one and the same specimens of steels submitted to different treatments, showed that the condition in which the carbon existed in the metal, whether in large or small proportions in the form of carbide, or in the more direct union with the iron throughout, seemed to have an influence upon its properties, and to be to some extent a means of explaining the results obtained. Among the specimens with which he had been furnished by Mr. Paget in carrying out his investigations were some samples of blue steel, which had been heated only for fifteen minutes, while others had been heated for six hours, and he had found that the proportions of carbide of iron existing in the specimens bore a direct relation to the period during which they had been exposed to heat. In the case of two specimens of the same metal, one of which had been heated fifteen minutes to a blue heat, and the other tempered for six hours, the first yielded 2.37 per cent. of the carbide, and the second 4.61; there was, therefore, an increased proportion of the carbide formed, and possibly an increased effect in the direction of want of homogeneousness of the metal consequent upon a longer exposure to a blue heat. In another set of experiments, the blue steel, which had been treated for fifteen minutes, furnished 3 per cent. of the carbide, while a portion of the same steel which had been heated for six hours furnished 6.6 per cent. The specimens were disks that had all been cut from the same piece and treated exactly in the same way. In a third set of experiments he obtained corresponding results. The specimen which had been heated fifteen minutes furnished 3 per cent., while that which had been heated for six hours furnished 6.6 and 6.76 per cent. The facts that he had mentioned might perhaps add somewhat to the information which the discussion was intended to elicit.

Mr. B. Baker had observed a characteristic feature in the discussion which he regarded as a hopeful one. Every speaker had taken the trouble to make experiments, and if that process was continued he believed engineers would soon come to the bottom of the so-called mysterious fractures of steel. It was no use to theorize without plenty of data. The author had set an excellent example, his paper consisting almost entirely of a record of experiments, leaving others to contribute additional facts bearing on the subject. The result he hoped would be that there would be shortly an accordance as to the causes of the fractures. His own experiments had been rather considerable, for at the Forth bridge about 1,000 tons of steel girders were now being turned out per month. A considerable proportion of the steel had to be heated, and it was liable to such contingencies as the author had properly stated it was absolutely essential to guard against. He agreed with the author

in his conclusions as to the result of bending plates cold after they had been subjected to a blue heat; but it had occurred to him that in practice no plate would ever be liable to be bent to an angle of 40° or 50° if it had once got into the structure. What it was perhaps liable to, especially in the case of boilers, was the frequent repetition of stresses beyond the elastic limit. He had therefore made some experiments in that direction with flat bars treated in different ways. He first took a flat bar and annealed it, and tried how many times he could bend it backwards and forwards, putting the extreme fibers into alternate tension and compression, amounting to about 20 tons per square inch. He found that a bar which had been annealed stood from twelve thousand to eighteen thousand repetitions of a stress of that intensity. He next took a bar which had been bent when hot to an angle of 85°, and he straightened the bar at a blue heat, and put it so straightened into the machine, and he found to his surprise that so far from materially falling off in the number of repetitions which it endured it stood over sixteen thousand. He then took another bar and put it into the machine red-hot, and it was bent through all ranges of temperature from a red heat till it was quite cold. That bar stood about twenty-eight thousand repetitions, and another bar which had been bent four times at a blue heat stood thirty-two thousand. Every one of the bendings bent the bars beyond the elastic limit to the very severe stress of 20 tons per square inch. That could not, he thought, occur in a bridge, but it might conceivably occur in a boiler if the door were opened and closed, and a current of cold air struck the combustion chamber. But the fact was indisputable that the bending at the blue heat did not reduce the amount of fatigue which the steel would undergo. He did not infer from those experiments that it was unnecessary to guard against injury in the way indicated by the author. He only wished to point out that it should not be supposed that failure was imminent if by chance a bar which had been bent at a blue heat and had not been annealed got into a structure. Whatever precautions might be taken exc·ptional cases would occur; but his experiments had shown that the danger was not so great as the author's experiments appeared to indicate. The author had dealt with one of the causes of mysterious fractures, and one alone; he had not touched upon the cause which the late Sir William Siemens almost always brought forward, viz, a small crack which started a tear. Sir William often likened the behavior of steel to that of a piece of india-rubber; but if he had tried the experiment he would have hardly drawn that comparison. It was extremely dangerous to indulge in analogies without making the proper tests. Mr. Baker had compared steel with india-rubber experimentally, and he found that they behaved in entirely different ways, and for obvious reasons. India-rubber was a material which would stretch within the elastic limit 400 per cent.; mild steel was a material which would stretch within the elastic limit only one-seventh of 1 per cent. On the other hand, india-rubber had a set of only

about one-sixth, whilst steel had a set of one-fourth or one-fifth. The curves of elongation were entirely different. The curve of steel was a very flat incline, and then a sharp curve upwards, but the curve of india-rubber was a reverse curve. If an examination was made of the characteristic curves of steel and rubber in relation to the explanation of Sir William Siemens as to mysterious fractures, it would be seen that it was an all-essential element in regard to anything of the character of a tear. So far, as to the known characteristics of the materials as illustrated by the curves of elongation. But going to absolute experiment, he had found that by making a slight tear in india-rubber the strength of the remaining part of the section was reduced by 60 or 70 per cent. with a fair pull. In the case of one specimen of india-rubber, a fine cut was made on each edge with a sharp knife; it was then loaded, and the strength of the remaining uncut section was only from 30 to 40 per cent. of the original strength per square inch, showing a loss of from 60 to 70 per cent. He did the same thing with steel, making first a fine saw-cut on each side of the specimen, and then raising the specimen nearly to a welding heat—to such a heat that he could close up the saw-cut and render it invisible, so that it was to all intents and purposes a hidden crack. He then put it into the testing machine, and instead of falling to 30 or 40 per cent. of the original strength of the material like india-rubber, the result was 31.4 tons per square inch (Figs. 1 1*b*,), the strength of the original bar (*a*) being 32.5 tons, showing a loss of only 1 ton per square inch from the presence of the cracks. Not only, however, was the strength maintained, but the character of the fracture was different in the two materials. In Figs. 11, *g*, a star-shaped fracture

Figs.11.



would be seen. It might be thought that this specimen was a piece of brittle steel, but it was india-rubber. The fracture of a steel specimen

was very different; there was a measurable elongation, which could be calculated, and a reduction of section. He had also tried bars, prepared in the same way, but with a fine saw-cut on one side only closed under a temperature less than welding heat. The resistance was 24.7 tons per square inch (*c*), when the bars were held in the usual way in jaws in the testing-machine, and 16.9 tons when held by a pin. The form of the fracture in the first instance was as represented in *h* and *l*; there was an elongation and a gradual reduction of section of increasing amount from the bottom of the crack towards the opposite edge of the bar. Analogous fractures occurred (*k*) with the bars cracked on both edges. In some discussions at the Institute of Naval Architects it was maintained by several speakers, including Sir William Siemens himself, that in the case of a tear there could not be a reduction of section, but in the instances he had referred to it was so. Of course, when a fracture took place in a steel bar it usually occurred too quickly to observe the process. In order to follow it the best way was to make specimens in lead. He had adopted that plan, and was thus enabled to see how the reduction of section happened. The elastic limits and the ultimate strength held about the same ratio to each other in lead as in steel. Soon after the elastic limit was passed the crack opened, and became rather parabolic in form, the metal thinning rapidly at the bottom of the crack (Figs. 11, *m*). As the tear proceeded a wave of thinness passed along the bar from the crack to the opposite edge, the minimum thickness always being at the bottom of the tear, and growing less as rupture extended. It had long been known that if a bar was nicked with circular nicks the strength of the remaining section was greater than that of the original bar. The tensile strength of the steel in the case to which he was referring was originally 32½ tons per square inch in the plain bar, and it went up to 36.3 tons with the semicircular nicks (*d*). He had also tried the experiment of putting a ½-inch hole through the specimen and making a slight nick on each side of the hole, forming, as it were, a badly-punched hole, so badly punched that the bar was torn for about $\frac{3}{16}$ inch on each side; and even under those circumstances it was not by any means destroyed. The only result was that the strength of the section left was reduced to 28 tons to the square inch (Figs. 11, *e*). He had taken the same kind of specimen and raised it to a blue heat and bent it six times backwards and forwards; but instead of snapping when tested for tensile strength, it sustained 34½ tons to the square inch—more than the original plain bar, which had not a nick in it and had not been tampered with at all. Plain bars bent several times at a blue heat assumed the barrel-shaped outline already referred to by Professor Unwin, and stood 34.4 tons (*f*). It had been said that steel and iron were entirely different, that steel was a homogeneous material and iron a bundle of fibers. The author had shown that steel and iron at a blue heat behaved much in the same way. Mr. Baker had shown by his experiments that all that had been

said about bundles of fibers, or what some persons preferred to call the molecular column, stopping the crack running through, was not justified by the facts. When he took a bar of wrought iron, nicked it on both sides, shut it up at a welding heat, or a little below, and put on the stress, it stood 22½ tons per square inch on the remaining section, the strength of the iron being 23½ tons, showing a loss of 1 ton to the square inch. The "bundle of fibers" did not help the iron in the least, and the steel was not hurt by being a very perfect material without any cinder between the layers. Each material lost in strength practically only 1 ton per square inch by initial cracks. He did not find that annealing had much effect in cracks of that sort. Of coarse the injury which steel received from shearing, whether due to cracks or not, was entirely remedied by annealing, and it appeared to him to be pretty conclusive that the injury received from shearing was not due to the initial cracks in a metal, because when he made initial cracks purposely he did not obtain that result. He exhibited a piece of steel which looked like a spring; it was a shaving from a 36-ton per square inch steel plate 1½ inch thick, the shaving itself being $\frac{1}{24}$ inch thick. The cracks were so numerous that the inside of the coiled shaving felt like a file, and not withstanding that, if he annealed the shaving he could flatten it out. He had made a saw-cut ⅛ inch wide in the side of a bar, and wedged it with a view to split the bar until the cut had widened to nearly 1 inch. He had partially torn bars before testing for ultimate strength, and found the same results as with saw-cuts. He had also cut a specimen from a cracked plate of 35-ton steel, so as to include the last ½ nch of an almost invisible crack, and tested that under tension, and found the crack did not extend until the stress on the remaining section attained 29 tons per square inch. It was indeed difficult to crack good steel, and a cracked plate generally meant a bad plate.

Mr. Jeremiah Head said he was one of those who had never ceased to wonder at the marvelous qualities of what was called mild steel, in comparison with the wrought iron which it had so largely superseded. But perhaps in view of those valuable qualities it was too much to expect that it would not have its own peculiar drawbacks. No rose, it was said, was without its thorn. He was not sure that it was quite creditable to engineers that they should not have found out long since and estimated at their proper value the very elementary qualities which they were then considering. There appeared to be a general concensus of opinion that to work steel at a blue heat was very dangerous, and his own experience led him to agree with that view. It was only the other day that he was passing by a vertical or stack boiler which was undergoing repairs. A ring had been made out of a steel plate, in form like an angle-iron ring, to effect the junction of the protruding flue with the flat plate forming the top of the boiler. The ring had been made when the material was at a full red heat, and with perfect success. It had been riveted to the flat plate at the top of the boiler also fault-

lessly, and it was now being riveted to the flue which went through. Some of the rivets had been put in, and he happened to notice one of those suspicious-looking marks which indicated that there had been some calking done between the rivet and the edge of the plate. That led him to examine it very closely, and he soon found that there were cracks from the holes in various places, not less than half a dozen. He pointed it out to the foreman, who said that when he was by the boiler a short time before there was not a crack in the ring. The fact was that it had not fitted the flue coming through quite closely, and the men had put heaters on, and no doubt heated it to a blue heat; then closing it with hammers, with the result he had mentioned. He had seen the same kind of thing before. A similar ring was made without any trouble whatever, but requiring a little adjustment after it was cold, it was put on a flat block and heaters were put on one or two places, and very slight blows given to it. Precisely the same result ensued. It cracked in two or three places and became useless. The remedy of course was, as he had pointed out in several instances, to work it either cold or hot, but not at a blue heat. One thing in the paper had struck him as contrary to his own experience, namely, the assertion that liability to brittleness at a blue heat was common to iron and steel. He had certainly not found it to be a peculiarity of iron, and he could give many proofs that the contrary was the case. Iron-plate boilers were almost always warmed by heaters at the places where an angle-iron or another plate crossed a joint, and needed a little setting in. When heated and closed in in that way he had almost always seen it make a good piece of workmanship, and had rarely seen any symptoms of cracking, unless attempts had been made to do this cold. Again and again when iron plates had been found perhaps rather brittle in bending, and especially when it had been attempted to bend them cold after punching, and perhaps a plate or two had failed, the rest being bent at a blue heat obtained perhaps by a fire of shavings, they were almost certain to bend right. One of the most difficult things was to get an iron plate, say $\frac{5}{16}$ inch thick, and rolled out so that its length was much greater than its breadth, to bend across the grain into a form like the mast of a ship, without cracking. In Holland it had been attempted to bend such plates into masts cold, and the number of failures was very great. Such plates were what was called "reedy," because most of the rolling had necessarily been in the lengthwise direction, and they were apt to split at the ends. But a very little heating, just taking the chill off with a fire of shavings, was generally sufficient to make them bend perfectly well. An engineer, who served his apprenticeship thirty or forty years ago, had probably as one of his first pieces of practical work to put studs into a cylinder. Being at that time made of iron, and when they were put in not always being quite perpendicular, it was customary to give them a blow with a hammer in order to bring them to a perfectly vertical position. Every apprentice knew that if he tried to do that

cold he was certain to break two or three of them, and then he had the disagreeable task of drilling out the stumps left in the hole; but if he obtained a big nut, or something of that kind, made it red-hot, and put it on the stud and worked it up to a blue heat, he could always set it with the greatest certainty and accuracy. Without, therefore, denying the author's experiments, he could only say that his own experience had been of a different character, tending to show that the particular drawbacks in iron were not those of steel. Steel was decidedly superior to iron, in the physical qualities of tensile-strength both ways of the grain, contraction of area, and elongation. In those qualities it would beat iron altogether. But in regard to the particular weak point of steel, its liability to brittleness at certain temperatures and to tear, and its liability to behave treacherously at times, it was certainly inferior to iron. Iron, as far as he knew. always showed its worst side first. There was at times trouble to work it up, and it occasionally exhibited defects which were very troublesome, leading to rejections during construction. But if it once entered into the construction it seldom deceived afterwards; whereas steel seemed to disarm all suspicion by the marvelous way in which it at first behaved, but then it seemed liable to behave in a treacherous manner afterwards.

Mr. W. H. BARLOW, past-president, asked if Mr. Head had made any experiments in testing steel with the grain and across the grain.

Mr. JEREMIAH HEAD replied that he had made such experiments, and that the results were in either direction practically the same.

Mr. SAMSON FOX said the time might be remembered when no one grumbled at paying £32 10*s*. per ton for the material with which to make certain boilers, but now there was a great deal of grumbling if £8 a ton were asked for it, and people expected the £8 per ton material to serve with as small a percentage of difficulties as attended the other. Unless engineers were prepared to begin with a high-class raw material, the very best that could be obtained, and to use it by the new process, they would not escape with the low percentage of failures as they might otherwise. He had dealt with between three and four thousand plates during the past year in smithing operations; they had all been intricately formed, and in no single instance had he experienced any of the trouble that had been referred to in the discussion. He had found, like Mr. Traill, that the less he had to do with plates made for ordinary commerce, where there was a considerable bickering about the price, the better were the results obtained. The employment of those plates would entail considerable difficulty in the course of the work. With the finest pig metal used in a careful way the best results were secured, but the other course would entail loss.

Mr. J. T. MILTON said the author had asked him to accompany him that morning to see the effect of blue heat on some mild basic-steel angles. The experiments showed that a piece cut cold from the angles stood, in the way he had already described, thirty-one bends before

breaking. A piece cut from the same angle drawn out cold stood the same number, thirty-one, and when drawn out red-hot it stood forty-one. The pieces which had been drawn out at a blue heat broke at the first bend when tried cold, while a third one stood fifteen bends; when bent at a blue heat, one piece stood two and a half bends, one three bends, and another four bends. The effect of working steel at that heat was that what was ordinarily called ductility was entirely taken away from the metal. In his opinion the temper-test was a very important one. A few month ago he had made some experiments on different qualities of steel with reference to that very point. In the case of very mild steel of 25 tons original tensile strength, when made red-hot and quenched the strength was raised to 38 tons per square inch. Whereas it originally stretched 24 per cent. in 10 inches, after quenching it only stretched 10 per cent. In the case of harder steel of 29 tons strength, after the quenching the strength went up to 43 tons, and the ductility diminished from 23 per cent. to 5 per cent. Still harder steel of 35 tons tensile strength, which when annealed had an elongation in 7 inches of 22 per cent., when quenched went up to 50 tons, with no elongation whatever. The quenching was done in water at a temperature of 80°. When quenched in soapy water, the hard Siemens steel, instead of standing 50 tons, only stood 43 tons, but still it left 11 per cent. extension; so that after quenching in soapy water it would no doubt stand considerable bending, whereas by quenching in cold water it would not do so. Mr. West had stated, in reference to the unexplained manner in which some steel boilers had failed, that he thought there must be a personal element in the testing of the original plates, because he had found that the tests made after the failure did not agree with the original tests. The original strength of the steel was between 26 and 29 tons, but after the failure it ranged from 24 to 40 tons. Mr. Milton thought it possible that either in the making of the boilers, or in their use, or in something which had happened to them from the time when the steel was delivered until the lapse of the two and a half years in which they were in use, the part of the plate that showed a strength of 40 tons might have undergone treatment somewhat analogous to what was called the tempering test, being made hot and suddenly cooled, which would fully account for the 40 tons without throwing any stigma upon the tester. It had also been stated that while the steel in question was being tested before it left the works there were many rejections. That showed that the tester was up to his work, and doing it properly.

It ought to be clearly remembered that the strength of steel, as shown by test-pieces, was not the useful strength of the material, it was not the strength which might be expected to be got in the structure. It might be said that steel had an elongation of 24 per cent., but the useful strength was attained, as Mr. Baker had said, when the elongation was only one-sixth of 1 per cent., that was, when it reached its elastic limit. In his experiments some very mild basic steel of an original strength

of 25 tons had an elastic limit of 17 tons only, and that was the useful strength of the material when put into a structure. Steel of 29 tons strength had an elastic limit of 18 tons, and 35-ton steel had only an elastic limit of 20 tons, so that in going up from 25 tons to 35, the increase in usefulness was only from 17 to 20. He thought it was doubtful whether, in such intricate forms as existed in boilers, and forms that were generally acknowledged to require a great deal of ductility in the material, it was desirable to insist on these high strengths, seeing that so little was gained by it, and that questions arose referring to hardening and tempering, which were absent in the case of mild steel.

Mr. W. PARKER observed that it had been intimated to him by one of the members of council that, in order to further clear up this question of steel becoming brittle when worked at a blue heat, it was desirable that some experiments should be made on a more extensive scale; and he had taken the matter in hand as far as time would admit, and had brought with him some pieces of steel which he had been testing with that object in view. The piece of plate which he tested was of a mild description, and of the best quality that could be produced, for the inside of a boiler. It was 4 feet long by 3 feet 6 inches wide by $\frac{7}{16}$ inch thick, and he had had it cut into three pieces. Those pieces were first of all put into an annealing furnace and annealed, in order to remove any injury that might have been caused by shearing the edges. The first piece had four strips cut from it. The first of these strips was bent cold to an angle of 45°, and then reversed and bent again to the same angle eight successive times (four one way and four the other), without any appearance of fracture. The next strip was made blue-hot, and in that state it was bent in the same manner, but it only stood three and a half bends before breaking. The next piece, number 3, was treated in like manner, and it only bent two and a half times before breaking. Piece number 4 was heated to a blue heat, and then hammered with a light steam-hammer in order to put some work upon it at that heat, and it bent four times before breaking. It was allowed to get cold before bending, so that there could be no doubt as to the cause of the injury being the work put on it at blue heat. Those tests to a certain extent corroborated the experiments made by the author, but he thought he would go a little further, and try the effect of work being put upon the other two pieces of plate in the same manner as would occur in ordinary practice. The second piece of plate, numbered 5 in the sketch, was made blue-hot and passed through ordinary boiler-makers' rolls until it was bent to the curvature shown by the specimen. It was bent to almost a circle, about 19 inches in diameter. It was then put back into the rolls again, and bent to the same curvature the reverse way, and there was no sign of fracture observable. The other piece (No. 6) was bent in like manner to the curve shown. The rolls were not small enough, nor close enough together, so that the piece could not be bent perfectly round. It was then turned the other way and

bent to the curve shown, and there was no sign of fracture. Both the specimens were bent at a blue heat. Those experiments went some little way to show that, in ordinary practice, the material was not injured to the extent that it had been injured in the smaller experiments. The work put upon the small strips was of a much more severe nature than the work that had been put upon the broader plates in the same condition. In his opinion steel was injured when work was put upon it at a blue heat, but the extent of the injury all depended upon the amount of work put upon it. He should like to see this matter thoroughly cleared up, and he therefore proposed to make some further experiments, with a view of ascertaining the amount of injury done, with a certain amount of work.

Mr. JOHN HEAD said that the author had referred to some mysterious failures in steel, and upon that point he wished to make a few observations. In his experience in steel manufacture, which had been somewhat extensive, he had found that up to the end of 1883 it was impossible to make an ingot of steel that should be absolutely free from blow-holes. He would ask the members to assume for the present that such was the case, and that the blow-holes would not be distributed uniformly in each ingot. When the ingots were rolled into plates the blow-holes still existed, although compressed, and they formed points of weakness in the plates. Owing to that cause sometimes test-pieces taken from the same plate would support different tensile strains. All of them would stand the same twisting and bending test, but the elongation might be different. In the tests mentioned by Mr. Baker some of the strips were found to support 24 tons, others 28 tons, and others 31 tons per square inch. He accounted for that by supposing that they had been cut from plates containing more or fewer blow-holes. There were several cases in which boiler-plates had been found to break in a mysterious manner, which he accounted for by supposing that the blow-holes had been distributed in a line in the plates. When such plates were riveted at the ends in a line with the blow-holes, of course the tensile strength would not be materially affected, but when riveted at the sides, crossways, a certain stress would be produced which would lead to breaking. He might be told that fracture had not occurred when the rivets were put in, but only after the men had left the work. That he believed was usually the case. He would point out that when the rivets were put in they were in a heated condition, and heated the plate, thereby causing a certain amount of expansion which was sufficient to allow for the stress, but when cooling took place stress was put upon the plate, and it snapped across in the line of blow-holes, but not necessarily through any rivet-holes. That blow-holes existed in steel could be proved, if not from steel itself, at all events by means of glass. He had with him bottles which had been made some years ago of ordinary glass, and the mark of blow-holes, technically called "seedy boil," could be seen in them. The same thing occurred in steel

ingots. In superior qualities of glass, like plate glass, the "seedy boil" was got rid of. After the glass was melted it was allowed to rest without flame in the furnace for three or four hours before it was poured, and during that time the "seedy boil" disappeared. It was of course impossible to deal with steel in that way, and the holes therefore must remain in all steel melted with contact of flame. Those holes were due, as had been proved in glass-making, to the flame striking on the metal, as thereby a certain amount of gaseous matter got into the glass; and it was the same with steel. In steel making there were what were called "boily" ingots. If the steel were homogeneous and free from blowholes, instead of boiling at the top of the mold, which necessitated stoppering with sand and wedge-plates, it should on cooling do what the men called "pipe"—that was, actually contract to form a hollow at the top, but it was seldom that it did that, unless melted without contact of flame. He had been led to the consideration of the subject by Mr. Frederick Siemens' mode of heating by radiation, in which there was no contact of the flame with the steel or the glass in the furnace. He exhibited some bottles that had been made in a furnace heated by radiation, and it would be seen that they were free from the defects to which he had alluded. Mr. Gilchrist had expressed the opinion that the presence of a certain amount, 0.05 per cent., of phosphorus in steel was rather favorable than otherwise. Sir William Siemens had made some experiments on that subject in the year 1868, and Mr. Head shared in them. They made some steel containing a certain amount of phosphorus, and they found that it broke, as it was called, cold short. If a bar of steel containing phosphorus were indented with a chisel and broken on an anvil, instead of following that line, which should be the line of least resistance, it would probably break off only partly along that line or altogether along some other part; so that he was disposed to think that phosphorus would not be a desirable ingredient to import into steel.

Mr. W. STROUDLEY wished to give the results of some experiments made, at the request of a member of the council, with some samples of Siemens-Martin steel, such as he used in the construction of locomotives, and specimens of which he submitted.

First series.—No. 1 was a piece of plate 5 inches in width, $\frac{5}{16}$ inch thick, made to his specification, from steel of 28 to 30 tons tensile strength; it was bent to an angle of 45° over the edge of an anvil, in the way described by Mr. Parker; and it was then bent the reverse way through an angle of 90°; and this was repeated eleven times (making a total of twenty-three times bending through an angle of 45°). It had not quite broken asunder; the fracture, instead of taking place at the edge and breaking inwards, as might have been expected, was more in the center, the edges remaining intact. This piece was taken as it came from the manufacturer, and was bent cold. No. 2, another piece of the same plate, had been heated to a blue heat, and bent to an angle of 45°

once, and then bent the reverse way through an angle of 90°. After cooling it was again bent; but it would only bear bending ten more times as compared with twenty-three times of the cold plate that had not been interfered with. The fracture extended to about the same point, the edges remaining whole as in the other specimen. No. 3, another piece cut from the same plate, was heated to redness, and bent once to an angle of 45°, and again through an angle of 90°, in precisely the same manner. It was then allowed to cool, when it bore twenty-three additional bendings through an angle of 45°, the fracture being somewhat less than in the other instances, showing that that treatment had made it stronger than in the case of the steel which had been bent when cold, or at a blue heat.

Second series.—Other specimens were taken to test the tensile value of this material. No. 1, a piece of steel in its original form as delivered from the manufacturer, broke at a tensile strain of 30.5 tons, with an extension of $\frac{7}{16}$ per inch. No. 2, after being heated to a blue heat, was allowed to cool, and was then tested. It broke at 29.4 tons, showing an elongation of $\frac{5}{16}$ per inch. No. 3 was bent once through an angle of 45°, and was then straightened by hammering; when tested it broke at 33 tons, showing an elongation of $\frac{3}{32}$ per inch. No. 4 was heated to a blue heat, and bent through an angle of 45°, then straightened and allowed to cool, and was afterwards tested, showing a strength of 32.2 tons, and an elongationof $\frac{1}{32}$ per inch only. No. 5 was heated to a red heat, and allowed to cool, and was then tested. It broke at 29.4 tons, showing an elongation of $\frac{3}{8}$ inch. No. 6 was heated to redness, bent to 45°, and straightened; and when cold was tested, showing a breaking value of 29.8 tons, and an elongation of $\frac{5}{16}$ to the inch.

Third series.—Pieces of steel, 14 inches long by 4 inches wide by 1 inch thick, were cut from framing-plates, the edges of the plate being planed smooth. No. 1 was bent whilst cold to an angle of 45°, then laid aside to cool from the temperature caused by the first bending; it was then bent through another 45°, or equal to 90°; again laid aside to cool as before, then bent another 45°, or equal to 135°; cooled as before, and finally folded close without fracture. No. 2 was bent whilst at a blue heat to an angle of 45°, and allowed to cool; then bent to an angle of 90°; then to 135°, and finally to within $\frac{3}{16}$ inch of being folded over close when it broke on the inside edge of one fold, the fracture being $\frac{1}{8}$ inch open. This specimen was also allowed to cool between each bending. No. 3 was bent whilst at a red heat to an angle of 45°, and allowed to cool; it was then bent to an angle of 90°, then to 135°, and finally folded close when it was slightly fractured on the inside edge of the plate. No. 4 was heated to a bright red and allowed to cool. This was folded close, and was slightly fractured on the inside of the plate. No. 5 was bent while at a blue heat to an angle of 90°; then made a bright red, put into hot ashes and allowed to remain until cold; it was afterwards folded close to within $\frac{3}{32}$ inch, when it fractured on both sides.

No. 6. Two plates were bent at a blue heat to an angle of 90°; then made a bright red and placed aside in a cold atmosphere; they cooled rather quickly, and were afterwards folded close without fracture. No. 7 was bent while at a blue heat to 90°; then made a bright red and put into boiling water. When it had cooled to about 200°, it was placed in a pail of hot water, and the pail put into a tank of cold water. It was folded over when cold quite close without fracture.

Fourth series.—The specimens submitted to these tests were cut about 8 inches square, from Siemens-Martin steel frame-plate 1 inch thick; the edges were planed smooth. In each corner a hole 1 inch in diameter was drilled, the center of which was $1\frac{3}{4}$ inches from each edge. These holes were drifted out with drifts having $\frac{3}{64}$-inch taper per inch of length. No. 1 specimen, as from the manufacturer, was drifted whilst cold until it fractured; one hole being increased from 1 inch to $1\frac{15}{16}$ inches in diameter, and the other hole to $2\frac{1}{16}$ inches. No. 2 specimen was as before, but tested to a blue heat, the holes being drifted to $1\frac{1}{2}$ inches in diameter. It was then laid aside to cool, and afterwards tested; when opened only $\frac{1}{8}$ inch more the specimen split across, showing very brittle fracture. Other experiments were made in a similar manner, the fractures occurring at $1\frac{9}{16}$ inches, $1\frac{13}{16}$ inches, $1\frac{9}{16}$ inches, $1\frac{33}{64}$ inches, and $1\frac{5}{8}$ inches diameter of holes. No. 3, as before, was raised to a red heat, and the holes were drifted to $1\frac{1}{2}$ inches in diameter. It was then allowed to cool, and was afterwards drifted until one hole fractured to the extent of $\frac{1}{16}$ inch open, it being $2\frac{1}{2}$ inches in diameter; the other hole was drifted to the same diameter without fracture. No. 4, as before, was raised to a bright red heat, and allowed to cool. It was then drifted until fractured; one hole was $2\frac{7}{32}$ inches in diameter; the fracture was $\frac{1}{8}$ inch open. The other hole was $2\frac{3}{8}$ inches in diameter, and the fracture $\frac{1}{16}$ inch open. No. 5 was drifted at a blue heat until it fractured. The diameter of the hole was $1\frac{29}{32}$ inches; the fracture was $\frac{3}{64}$ inches open. No. 6 was drifted at a blue heat to $1\frac{1}{2}$ inches diameter, then made a bright red, placed in hot ashes, and allowed to cool. It was afterwards drifted until fracture occurred, one hole being $2\frac{5}{16}$ inches in diameter, with the fracture $\frac{3}{64}$ inch open, and the other hole $2\frac{3}{32}$ inches in diameter, with the fracture $\frac{1}{16}$ inch open. No. 7. Two plates were drifted at a blue heat to $1\frac{1}{2}$ inches in diameter, and then made bright red. Being placed aside in a cold atmosphere they cooled rather quickly. They were afterwards drifted until fracture occurred, the holes being respectively $2\frac{11}{32}$ inches in diameter, the fracture $\frac{1}{8}$ inch open, and $2\frac{9}{32}$ inches in diameter, and the fracture $\frac{3}{64}$ open; and $2\frac{1}{2}$ inches in diameter not fractured; and $2\frac{11}{32}$ inches in diameter, with the fracture $\frac{3}{64}$ inch open. No. 8 was drifted at a blue heat to $1\frac{1}{2}$ inches; then made a bright red and put into boiling water. When it had cooled to about 200°, it was placed in a pail of hot water and the pail put into a tank of cold water. One hole was then drifted cold to $2\frac{1}{2}$ inches diameter, and was not fractured; the other hole was fractured at $2\frac{7}{16}$ inches diameter. No. 9 was drifted at a blue heat to $1\frac{1}{2}$ inches diameter,

then made a bright red and put into cold water. One hole only was drifted when cold to $2\frac{1}{2}$ inches in diameter.

Fifth series.—These tests were made with pieces of Siemens-Martin steel, sheared from frame plates, having a 1-inch hole drilled in each corner; the centers of the holes were $1\frac{3}{4}$ inches from the nearest edges. These plates were sheared with ordinary scrap shears. No. 1 specimen was drifted whilst cold until it fractured. The first hole was $1\frac{19}{32}$ inches in diameter, the next $1\frac{5}{32}$ inches, the third $1\frac{7}{16}$ inches, and the fourth $1\frac{9}{32}$ inches in diameter. No. 2 was drifted at a blue heat to $1\frac{1}{2}$ inches and allowed to cool; it was then drifted to $1\frac{3}{4}$ inches in diameter, when it fractured, the fracture being $\frac{1}{16}$ inch open. No. 3 was drifted at a red heat to $1\frac{1}{2}$ inches and allowed to cool. It was then drifted until fractured, one hole being $2\frac{1}{4}$ inches in diameter, with the fracture $\frac{1}{8}$ inch open, the other being $2\frac{3}{8}$ inches in diameter, with the fracture $\frac{1}{16}$ inch open on the inside of the hole. This occurred in this case only. No. 4, after being raised to a bright red heat, was allowed to cool, and then drifted until it fractured. One hole was $1\frac{15}{16}$ inches in diameter, the fracture being $\frac{1}{8}$ inch open, and the other hole $2\frac{1}{16}$ inches in diameter, the fracture being $\frac{1}{16}$ inch open.

With a view to determine the injury, if any, caused by working Siemens-Martin steel of 28 to 30 tons tensile strength, such as would be be used for marine boilers, engine frame-plates, &c., several tests were made, which were thus arranged: 1. A specimen plate, as delivered from the maker, was tested in each of the several series, in its natural form. 2. Another specimen from the same plate was tested whilst at a blue heat. 3. Another specimen, after having work put upon it, was tested at a blue heat. 4. Another specimen was tested at a red heat to one-half the test, the finish being whilst cold. 5. Another specimen was tested after being heated to redness, and allowed to cool, so as to anneal it.

The behavior under these various conditions of bending, testing for tensile strength, and spreading out by punching a drilled hole to about double its diameter, went far to show that the views of the author, that the elasticity of the steel was permanently injured by being worked at a blue heat, whilst it was not injured by being worked cold or at a red heat, were correct. It would also be noticed that the steel was not injured by being heated to a bright red and plunged either into hot or into cold water. He must, however, direct attention to the fact that the specimens which were tested in this case were of the highest quality, by the best makers in Yorkshire. The result of his experiments, as far as he could judge, supported the idea that working steel at a blue heat did permanently injure it. Hitherto he had been of opinion that it was best and safest to work at a blue heat; but he was now satisfied that it was not so, and he thought that great credit was due to the author for having brought the matter forward.

Mr. W. R. HODGE had been for nearly forty years engaged in the practical operation of making boilers and similar structures, and therefore took great interest in the subject of the paper. He had been particularly struck with the statement that blue heat was injurious to the working of Lowmoor or superior Yorkshire iron. For years the practice with boiler-makers had been to put a large piece of red-hot iron on plates requiring closing, and heat them to a temperature that was now considered dangerous, about 600° Fahrenheit, and during all his experience he had never seen any very serious consequences result therefrom. As the matter was one that interested all persons connected with engineering, he had resolved to try an experiment, and he accordingly cut four pieces, two from Bowling plate, and two from a steel plate, both 2 inches wide, and $\frac{5}{16}$ inch thick, and he submitted them to exactly the same treatment. They were heated to a blue heat, about 600° Fahrenheit, as far as he could tell without a gauge, and they were bent over the hammer block of a steam-hammer at right angles, then straightened at the same temperature, and then bent again and again till the samples broke. He then heated the other ends of the same pieces to blue heat, and hammered them under the same hammer, in order to flatten them to the extent of 20 per cent., so that the $\frac{5}{16}$ inch was reduced to $\frac{1}{4}$. He allowed those pieces to get cold, and twenty four hours afterwards he submitted them to same kind of treatment that he had given to the others. The result was that the Bowling iron went down very much in strength and value, while the steel if anything increased in strength and value after the hammering. He then tested in the same way two independent pieces, bending them cold, and the hammered and in the Bowling iron went down in strength and value, but the steel did not. The steel was manufactured by a well-known Scotch firm, and he thought it was of a fair average quality of Siemens steel. The firm were as good makers as any he had known. The only fault he had to find with Siemens steel was the difficulty in welding, a difficulty that still existed. In cooling incipient cracks parallel with the scarf were developed. Every boiler-maker, and every one engaged in that kind of business, could not but rejoice at the extreme homogeneousness of the material. It was now a real pleasure to execute difficult flanging, and other work of that kind, with so excellent a material as compared with iron, even of the best quality. The author had suggested the carrying on of the experiments a little further, by bending two pieces of plate of equal quality and equal size, if possible, to breaking, one being bent cold, and the other submitted to blue-heat or black-heat treatment. He exhibited two such specimens. One specimen was heated blue, and was bent three times each way in ordinary boiler-makers' rolls, and rolled up to a 1 foot 3 inches radius, then turned over and rolled up the reverse way to a 1 foot 3 inches radius at one rolling. The other was treated in the same way cold. Subsequently, when the specimen by contact with the rolls went down to what seemed to be less than 600° Fahrenheit, he increased the heat again by putting it into the furnace, and then sub-

mitted it to nine successive bendings each way, making in all twenty-four bendings at a blue heat. He treated the cold piece in the same manner, and there were no signs of fracture in it of any kind. It appeared, therefore, that although blue heat did deteriorate steel, it did not deteriorate all steel alike. There must be considerable differences in the qualities of steel, and he thought that it was very much more in the hands of manufacturers to produce a trustworthy article than it was in the hands of those who had to use the material. If manufacturers would address themselves to that point, he believed the problem would not be found so very difficult. His experience of the material in question did not accord with that of Mr. Jeremiah Head, who had stated that he had seen some of it almost go to pieces, as Mr. Hodge's firm had commenced making boilers with Siemens steel in 1879, and they had never known it go to pieces in that way; in fact, their experience had been all the other way, tending to establish the uniformly good and satisfactory character of the material. The only other difficulty, besides the welding, was that on very rare occasions (he only knew of two) the Siemens steel had come out a little laminated; but its homogeneousness, as compared with Bowling or Lowmoor iron, was remarkable. In days gone by, every third or fourth plate of Lowmoor or Bowling iron when severely worked was rejected on account of its unsoundness. He would recommend manufacturers to be particular in recording all failures, and the users of the material to tell the manufacturers of the same, so that they might address themselves to the task of finding out what was the real cause of the peculiar behavior of some steels, and endeavor to produce a material which would be as superior to Lowmoor iron as Lowmoor iron was to common iron.

Mr. E. A. Cowper thought that the pieces tested by the author were, perhaps, rather small for good practical proofs, but nevertheless he had been led to the conclusion that blue heat was injurious as a working temperature for iron. Mr. Cowper had always bent iron either cold or hot. When iron was hot it was in a malleable state something like wax, so that it could be bent backwards and forwards without a wound, and that was forging. When it was bent cold it took a set, and if it was bent backwards and forwards often it would crack. But bending iron at blue heat was neither the one nor the other. The material was not sufficiently soft to be malleable and wax-like, but it was stiff enough to be injured by the bending. Whether it was more injured by bending at a blue heat than by bending cold was entirely a question of experiment, and the experiments recorded in the paper seemed to point to that result. It was impossible to exaggerate the importance of the subject, because, if some expressions in the paper were to be taken as absolutely proved, engineers had all been wrong in their boiler-making and ship-building, and they could no longer treat plates in the way they now did. He alluded particularly to the intimations, to the effect that "steel plates and bars have failed," and with "little doubt at a blue heat." "All these results point unmistakably to the great danger"

at a blue heat. The plates "should under no circumstances be hammered or bent" at a blue heat. It was a "question whether one quality of steel is and another is not affected by long-continued exposure to a blue heat." And, finally, it being said to be clear that the "experiments" "show how steel can with certainty be made permanently brittle. Now, it was an undoubted fact that steel bent with less force when at a blue heat than it did when cold. Screw pins or studs had been bent when heated by a piece of red-hot gas-pipe, if put in at a wrong angle, but he preferred to drill the holes and tap them truly by letting the drill and tap be guided through a block fixed truly (as was the practice with some of the best engineers) to bending them after they had been put in wrongly. Mr. Cowper did not mean to say that steel-plates were not sometimes injured if worked at a blue heat; he believed that to some extent it was the case, and it would be most interesting to investigate to what extent this was so, and under what circumstances. He preferred to work a plate or even bend it, either when cold or at a red heat, and then anneal it; but in some cases hardening to a blue temper had the most beneficial effect in increasing the toughness of the steel, and at the same time its strength, to a marvelous extent. For hair-springs of watches and chronometers, and for coach springs of all kinds, the tempering to a blue-temper was the best treatment to which they could be subjected. A hair-spring often made eight thousand million vibrations in a life-time. Again in the use of hard drawn steel wire for plough ropes for steam ploughing, for pit ropes, and for piano wire, &c., the plan of hardening to blue temper, and then drawing, increased the toughness of the wire to a great extent, and the tensile strength from 34 tons per square inch to 75 tons, and if it was made still harder and drawn it took 108 tons per square inch to break it. The "Austrian music wire," that entirely commanded the market here for some years, was made in this manner, and an English firm having adopted the same plan, realized a large fortune, and now it was a common process. There were a few observations in the paper as to different qualities of steel standing different numbers of bends; but there was no specification of what those qualities were, and no analysis whatever, nor was the temperature of the "core-store" given. He thought that in an inquiry such as this, exact analyses should be given, and he begged the author to supply them. The author stated, for instance, that one piece of iron only stood a very few bends red-hot, and there could not be a doubt but that it was decidedly "red short," and probably contained sulphur. He once was supplied with some thousands of spokes, about 3 inches by $\frac{5}{8}$, so utterly "red short" that they would not bend when red-hot to a 2-inch radius; in fact they were utterly worthless at a red heat, and yet they would bend nearly double when cold. The qualities of iron were very various, as for instance in "boat-plates" (a miserable quality), "common iron," "Lowmoor," "cable iron," "Marshall iron," &c.; and the difference in their working was far more than was experienced in bending iron when hot,

or when at a blue heat. The experiments of the Franklin Institute were well known, though they were rather old now; but it was then found that iron was stronger for a tensile strain when kept hot in a melted lead bath.

Some years ago, before steel was made at Middlesbrough, iron so often contained a considerable proportion of phosphorus as to be very "cold short," and it was nick-named "Middlesbrough steel;" hæmatite had to be used with it, to make a good bottom flange in tension, to stand certain tests applied to single-headed rails. He was extremely obliged to those members who had taken up the idea of making practical experiments on a sufficient scale, as to the extent of the injury sustained from bending a mild steel plate at a blue heat, and for the samples of plates exhibited that had been bent backwards and forwards at a blue heat. Some of these samples showed no symptoms whatever of cracks or injury, while at the same time others indicated that some injury had been sustained in certain cases, and it was generally agreed that it was best not to work a plate much at a blue heat. He quite admitted that the mildest quality of steel plates should be used for the purposes of tension, in all large boilers carrying heavy pressure, and that it should not be attempted to get strength by adopting harder steel, with more carbon in it, although pieces of the latter would stand a higher tensile strain when proved in a testing machine. Reference has been made to some of the steel used in the Forth bridge being of 34 and 37 tons tensile strength. That steel, however, was all used in compression, not in tension. Mr. Baker wished him to mention this, and he believed he would not think of using such steel in tension, because it was too brittle for that purpose. For boiler-plates steel could hardly be too mild, a tensile strength of 25 or 26 tons per square inch was enough; certainly the strength should be under 30 tons. In all cases of tension it was advisable to have tough soft steel with great malleability; steel with a higher tensile strength might be used in certain situations where it was entirely in compression.

Dr. W. Pole, honorary secretary, said that as the discussion had diverged to a certain extent into the mysterious properties of steel generally, the members might like to know a fact that had been lately communicated to him by Dr. Percy, as to the extraordinary qualities shown by steel in the form of wire. Dr. Pole had occasion in 1862 to make some experiments on the strain of piano-fortes; and he found that the steel wire used for them showed a most remarkable tensile strength. It was well known that if wire of a certain length and a certain weight sounded a certain note (giving a certain number of vibrations per second), it must be subject to a certain tension; and testing in that way it was found that the piano-forte wire stood a strain of from 100 tons to 120 tons per square inch. He had mentioned the fact at this Institution,[1] but at that time no one had ever heard of such a strain being withstood by steel in any form, and the assertion was doubted, even by no less an

[1] Minutes of Proceedings Inst. C. E., vol. xxi, p. 242; vol. xlii, p. 202.

authority than Sir Joseph Whitworth. It was, however, tested and found to be true. Dr. Percy, who had taken a great deal of interest in the matter, had recently told him that he had some steel wire which was actually standing, as tested in the machine at Woolwich with great care, the extraordinary strain of 154 tons per square inch, and he hoped that Dr. Percy would publish the particulars. In corroboration he had heard from Messrs. Broadwood that during the last ten or twenty years they had found that piano-forte wire had been greatly increasing in strength. It was a remarkable thing that steel in that form should possess such extraordinary and almost unheard-of tenacity. He ought to add that the wire alluded to was by no means hard and brittle, as steel of very high tensile power often was. It was subjected in its use to severe usage. It was wrapped round a small pin which served for tuning, and it was turned into loops and subjected to twists, showing that it had a certain amount of ductility. The strings also received violent blows, which, however, appeared to do them no damage. Dr. Pole also thought it right to mention another remarkable quality shown by steel wire, namely, the very high range of its elastic limit. A piano-forte string was, when in use, strained almost to its breaking point, and yet the elasticity remained perfect, as was shown by the fact that the string would, when once tuned, stand in tune, *i. e.*, would receive no permanent elongation, for an indefinite length of time. Dr. Pole conceived that a form of the metal which showed these extraordinary qualities was well worth study, and that if the conditions which produced these qualities could be exactly determined, the investigation might throw more light on the properties of steel generally.

Mr. JAMES MANSERGH stated that he had frequent occasion to erect steam-boilers, and until recently had always specified iron, on account of the more or less serious failures that now and then occurred with steel. Gradually, however, the consensus of opinion appeared to be inclining decidedly towards the adoption of steel, and a year ago he ordered three large Lancashire boilers of that material, which were just ready to be delivered when the alarming report by Mr. Maginnis on the failure of two marine boilers appeared in "The Engineer." That report was a most disquieting one, as the steel plates of which the boilers were made had passed all the Board of Trade and Lloyd's tests, and stood without the slightest defect the ordinary work of the boiler-shop, welding, &c., and their collapse after two years' use appeared inexplicable. The paper under discussion had come most opportunely, and its main point, the danger of working at a blue heat, might, he thought, account for these and other failures. He had obtained the views and experience of the successful makers of the three boilers referred to, which might be as interesting and comforting to other users of steel boilers as they had been to himself. This firm had, during the last six years, made upwards of two hundred steel boilers, principally of large size, and in the last three years a proportion of twenty steel boilers to one of iron. In the first and second years they had several slight mishaps with Sheffield plates, by

flue-tubes cracking at the roots of the flanges during cooling; but since then they had not experienced a failure of any description. He had inquired specially as to their practice in working, and found that they heated the steel plates up to very nearly the white heat customary with iron, and ceased working as soon as it came down to a dull red, considering it dangerous to approach the blue heat referred to in the paper. The rule-of-thumb test suggested in the paper, by drawing a stick or hammer handle over the blue plate, and not ceasing to work so long as the mark glowed, was considered unsatisfactory by them, as this glow would appear when the steel had become much too cold to work with safety. In their opinion it was a dangerous rule to approve of. Prbo-ably the absence of failure in their boiler-making during the last four years might result from the precautions they had taken in this respect.

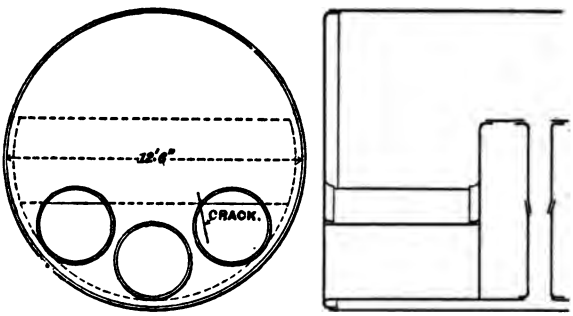
Sir E. J. Reed, K. C. B., M. P., thought the discussion had been a very valuable one, and full of interest to all, and to none more than to those whose experience of steel had been that it was incomparably superior to iron, but that, nevertheless, it had occasionally proved to be a material which it was rather difficult to understand. In ship-building he did not think there was so much trouble as in boiler-making; but in the establishments in which steel was first very largely used for ship-building, Her Majesty's dockyard officers and men became perfectly enamored of it as compared with iron, finding it much more trustworthy in every way, and much more capable of being dealt with reliably. Of course that did not apply to steel in the form in which it was known at first. He remembered a very striking instance of its treachery; some steel had been put into a stringer-plate in a ship, and it had been worked into place apparently very satisfactory, but, on looking the following morning, he found that it had cracked during the night, not through the rivet-holes or any point of weakness, but through the solid plate, owing solely to a change of temperature. But when the makers were limited to a low maximum of tensile strength, and produced the material now in general use, it was found to be of such a character that ship-builders would be exceedingly sorry to lose it. During the last three months he had closely observed the results of tests in steel manufactured for certain vessels under his care in six or eight manufacturing establishments, and in no case had it been necessary to reject a plate for its want of ability to conform to the rather severe admiralty tests. In fact, the introduction of steel had saved ship-builders nine-tenths of the trouble they used to experience in getting a satisfactory material, when they had to look for the best iron for a tensile strength of 22 tons per square inch, in conjunction with other good qualities. But the interest of the paper lay chiefly in the facts that had been developed, not when raising or lowering steel to a blue heat, but when putting work upon it in addition, and those facts had thrown a good deal of light on many of the incidents which ship-builders had had to observe. Generally speaking, so much satisfactory experience had been had with good mild steel in ship-building that most of the bending

had been done with the steel cold, no trouble being taken to heat it. It was extraordinary what an amount of shaping and handling it would take when cold, without apparently undergoing any deterioration. But it was not an uncommon practice to heat steel moderately, and up to what proved now to be an unsatisfactory blue heat, and while at that heat to put work upon it. There had been instances of comparative failure, and he believed that if they were examined it would be found that they grouped themselves around that condition of working with the blue heat. It was for that reason that he thought the paper would be particularly valuable to ship-builders, and every wise man engaged in the profession would pay careful attention to the paper, and to the discussion upon it—one of the most valuable he had heard on a technical matter. One point had not been noticed in connection with the many interesting and instructive experiments to which reference had been made. Statistics had been given of the angles to which specimens had been bent, and the number of times which they had been bent, but nothing had been said as to the degree of rapidity with which the change of form was brought about, and that, he thought, was a matter of considerable importance. A piece of plate might be subjected to those alternate bendings to a given amount, and yet afford very different results, according to the degree of rapidity with which the changes of form were effected. He agreed that it would add materially to the educative and, indeed, to the general value of the experiments which so many gentlemen had taken the trouble to make, if they would furnish full information about the material with which they had worked. Nothing was known of the chemical constituents of any of the materials used, and those who had listened to Sir Frederick Abel's speech must have been very much struck with the necessity of being acquainted, not only with the quantity, but also with the form in which the carbon entered into the structure of the material. The debate suggested the desirability of an elaborate course of experiments on the question. He thought there was no department of ship-building or boiler-making but would be benefited by a more complete series of experiments, attended by fuller information as to the material operated upon. He was sorry that he was unable at present to add any specific facts bearing upon the question. He had, however, derived a great deal of valuable instruction from the discussion.

Mr. STROMEYER, in reply, said he had tried to make his paper as short as possible by touching only one subject, though, of course, from his experience in going through boilers almost every day, he could have cited instances that had come under his notice which would have added interest to a discussion on the mysteries of steel failures, but he had thought it best to adhere as closely as possible to but one subject. As, however, the case of the Maginnis boiler had been mentioned, he might be permitted to refer to some failures in iron boilers that had come under his notice which closely resembled them. It was part of his duty to examine marine boilers at various periods of their life, and having

been requested to examine a pair of iron ones which had been built in 1878, according to the sketch (Fig. 12), and which had been out of use for about three months, he found that the back of the combustion-chamber had cracked 23 inches in the way shown in Fig. 13. In his mind there was no doubt that the failure had occurred within twelve hours previous to the survey. As the crack extended beyond a seam it could not have been due solely to bad quality of iron. In fact the iron appeared soft enough when chipped and drilled. That appeared to him to be quite as mysterious as any failure that had occurred with steel boilers. He knew of another case in the sister ship to the one he had mentioned where the top plate cracked, and also another where a crack

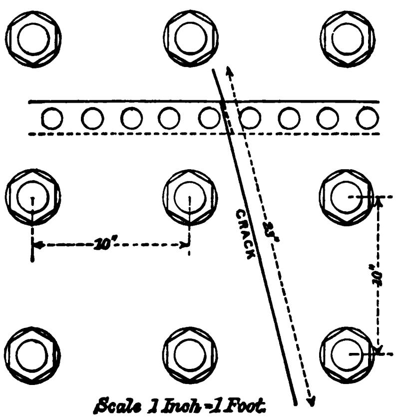
Fig. 12.



Scale 1/8" Inch=1 Foot.

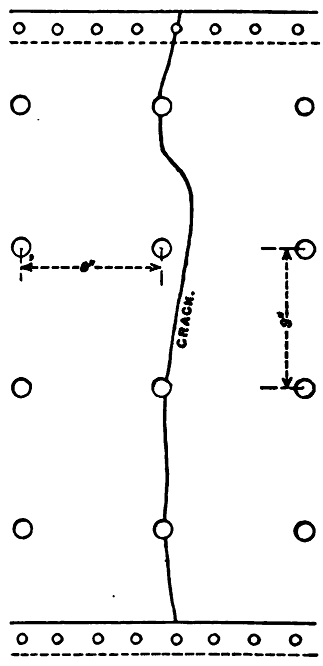
occurred as shown in Fig. 14. This one cracked during the dinner hour with a loud report about two weeks after the boilers were blown down. In the first of these cases the iron seemed perfectly good, so that there was a great amount of mystery attached to it. He did not think the steel boilers were any worse than iron ones. It

Fig. 13.



Scale 1 Inch=1 Foot.

Fig. 14.



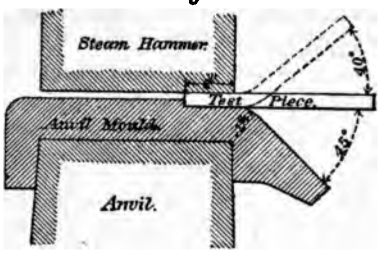
Scale 1 Inch=1 Foot.

would be noticed that in all these instances the backs of the combustion-chamber were flat and very large, as they were also in the boilers reported on by Mr. Maginnis, which would point to this form of construction being faulty rather than to the material being bad. Doubtless the steel, of which it was known that 40 per cent. had been rejected, and possibly the iron, were not as good as they ought to have been; and probably if the material of all these boilers had been of the highest quality even a more serious fault in the construction would not have produced such effects. It was not at all the object of the paper to explain such mysteries, but to show that steel and iron could be spoiled by a certain treatment. A favorite theory was that the strains set up by local heating caused subsequent failures of steel. It was said that if the corner of the plate was heated the unequal contraction of the plate would cause strains which would lead to breaking; but if the plates would stand 25 per cent. elongation in the testing-machine he could not see how they could crack. If any strains were set up the 25 per cent. elongation would allow them to ease themselves. But for all that there could be no doubt that cracks did occur. He remembered that some years ago a garboard plate had cracked in a very mysterious manner; he investigated it and found that by putting work on it at a blue heat he could permanently injure it. This was the commencement of the experiments submitted in the paper. One of his objects had been to discover the method of producing similar failures in test-pieces. He had not experimented with a view of showing whether the injury could be removed by annealing, but he had no doubt that this could be done. As an instance of how narrow the limit was within which steel was spoiled he would refer to Mr. Milton's experiments. Three test pieces were prepared by hammering them hot under a steam-hammer. Two of them broke with one blow of a hammer, while the other one stood fifteen bends. They were all prepared in the same manner, though one might have been a little warmer or a little colder than the others, or the hammering might not have been so severe. Whatever the differences of manipulation might have been they were certainly so small that the great difference in the result was very unexpected. He had tried to show how to spoil steel in order to warn every one against approaching that point. He had hoped that manufacturers of steel would be induced to make many more experiments, but he doubted whether curving large plates would give useful information. Curving was not as severe as the bending small test-pieces, and had led, as was shown by Mr. Hodge and Mr. Parker, to no results. It was evidently very difficult to spoil steel by workshop treatment, for many thousands of boilers had been made from it, and many plates must have been treated at a blue heat, yet mysterious failures were exceedingly rare. From the specimens he had shown, it would be seen that steel worked at a blue heat would stand two or three bends; this was even more than good iron would stand when it was cold. If he might suggest the direction in which further experi-

ments should be carried out, he would propose that manufacturers should make about a dozen bending tests under various conditions on all steel charges of which they had made a complete chemical analysis. A comparison of numerous results might show the influence of the various impurities on the blue-shortness of steel, and might point to a method of removing it. Another question, though of less importance, would then be to ascertain at what temperature the various steels would stand most work with the least amount of permanent injury. He then gave the following explanation of hardness, brittleness, ductility, and softness. The samples to which he had referred when broken at a blue heat could not be called brittle; he should rather call them rotten; they broke quite suddenly, and still they had silky fractures and not crystalline ones; but if worked at a blue heat and then allowed to rest, and then broken cold, they had crystalline fractures. Steel that had been prepared in this manner could be called brittle, but not hard, for it could easily be filed. Hardened tool-steel could be equally brittle, but it was much harder. He had measured the ductility by the number of bends; probably it could also be estimated from the amount of elongation or contraction of a test-piece. The softness would have to be measured by the amount of work required to bend the samples or to tear a test-piece. It was found to vary considerably in annealed and quenched mild steel, though the ductility was the same in both cases. Professor Unwin had stated that the stress-diagrams given in the paper might be incorrect by about 5 per cent., but as he had made all the measurements with an instrument that measured to the $\frac{1}{10000}$ inch, he doubted whether the errors could be even $\frac{1}{4}$ per cent. From measurements which he had made on the density of steel, before and after testing, he felt sure that it altered very slightly. He had not discussed any of the tensile tests for fear of lengthening the paper and prolonging the discussion on matters which did not properly belong to the subject. He had chiefly referred to the bending-tests, as they were the most practical ones. The other tests, however, were quite worthy of study. There was far more in the tables including the bending-tests and the diagrams than he had been able to explain, and if they were studied many points would suggest themselves which could be further experimented upon. He hoped that one day manufacturers would be able to produce steel that could not be injured at a blue heat. As it was, steel was one of the best materials for constructive purposes, and if steel could be produced which could not be injured by blue heat there would be nothing more to be desired, and he hoped that some important result in that direction might be attained. The manner in which the test-pieces, mentioned in Tables V to VIII, were bent would be seen from Fig. 15. An anvil mold was forged to fit the anvil-block of a steam-hammer, and shaped so that one end projected and was inclined to an angle of 45°; the bend was rounded off to a radius of 1¼

inches. The top of the mold was made $\frac{1}{8}$ inch higher than this radius, so that the test-pieces could butt against the ledge thus formed. When experimenting, the test-pieces were inserted as shown, and hammered down to the slant; it was, however, found that on account of the spring of the metal the bend only amounted to 40° and the radius of curvature

Fig. 15.



to $1\frac{1}{8}$ inches. The test-piece was then removed and replaced, as shown by the dotted lines, and the hammering continued. It took from two to four blows with a $\frac{1}{4}$-cwt. hammer to effect one bend. The quenched pieces required most hammering.

The following were the analyses of the steel and iron:

MEDIUM HARD STEEL (TABLE V).

	Test-piece.		
	No. 12.	No. 42.	Mean.
Carbon	0.294	0.292	0.293
Silicon	0.031	0.027	0.029
Manganese	0.640	0.630	0.635
Phosphorus	0.093	0.091	0.092
Sulphur	0.080	0.083	0.081
Copper	0.030	0.024	0.027

MILD STEEL (TABLE VI).

	Test-piece.		
	No. 25.	No. 43.	Mean.
Carbon	0.162	0.155	0.158
Silicon	0.021	0.015	0.018
Manganese	0.590	0.584	0.587
Phosphorus	0.060	0.063	0.061
Sulphur	0.060	0.072	0.066
Copper	0.024	0.016	0.020

LOWMOOR IRON (TABLE VII).

	Test-piece.			
	No. 1.	No. 14.	No. 42.	Mean.
Carbon	0.019	0.019	0.022	0.020
Silicon	0.125	0.155	0.129	0.136
Manganese	0.080	0.060	0.051	0.054
Phosphorus	0.089	0.076	0.068	0.078
Sulphur	0.009	0.007	0.004	0.007
Copper	0.040	0.027	0.030	0.032

VERY MILD STEEL (TABLE VIII).

	Test-piece.			
	No. 4.	No. 6.	Mean.	Charge.
Carbon	0.049	0.049	0.049	0.054
Silicon	0.012	0.017	0.014	Trace.
Manganese	0.176	0.145	0.160	0.170
Phosphorus	0.063	0.067	0.065	0.070
Sulphur	0.043	0.037	0.040	0.080
Copper	0.096	0.100	0.098	0.060

All the above analyses were made in one laboratory. The test-pieces which were drilled for analysis were selected from the extremities of plates used for experimenting.

An experiment, similar to those explained by Mr. Parker and Mr. Hodge, was carried out on an iron plate ¾ inch thick, 30 inches wide, and 4 feet long, which was sheared into two halves lengthways. One of them was twice curved when cold, and the other one when hot (blue to light straw), to a radius of 15 inches. The hot plate broke in the rolls. Subsequently test-pieces were cut about 12 inches from one edge of the plates and thus tested.

	Curved when cold.	Curved when blue-hot.
Unprepared	Bent to ¾-inch radius	Broke 20° bend.
Red hot, quenched in cold water	Bent double	Bent to ¾-inch radius.

The diagrams, Figs. 16, 17, 18, 19, referred to the tests in Table I. No analysis of these steels had been made.

Fig. 16.

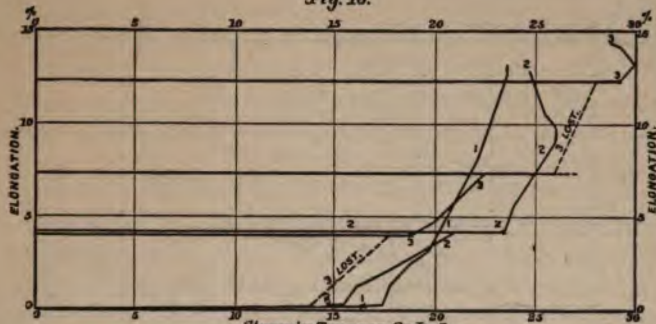


Fig. 17.

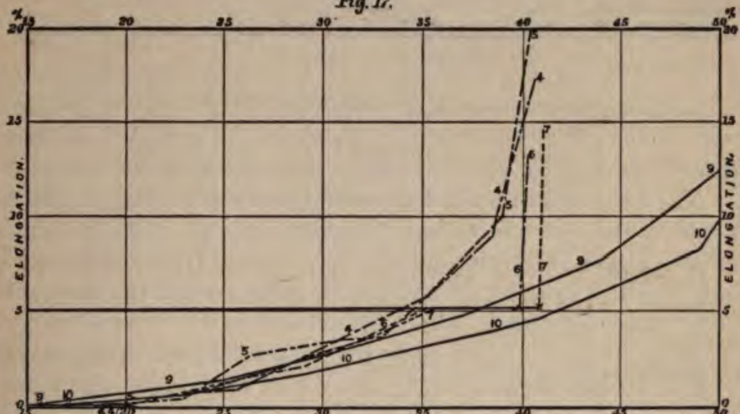


Fig. 18.

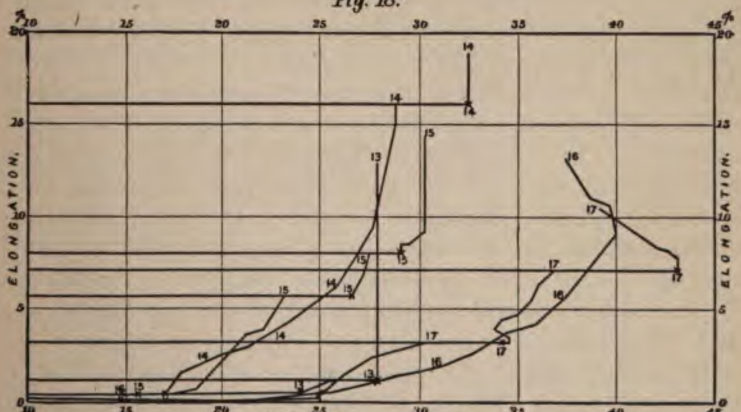
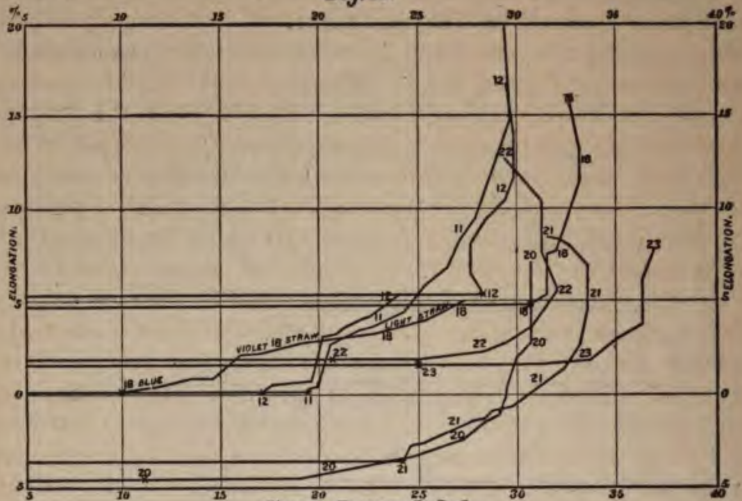


Fig. 19.



Sir FREDERICK BRAMWELL, president, said that the author had most modestly put forward his views, but he was quite sure that the members would feel that a more valuable paper had rarely been brought before them. The subject had been dealt with simply and clearly, both in the paper and in the author's verbal observations, and it had given rise to a discussion that he thought he had never heard surpassed in that room. The question of the effect of subjecting soft steel to the temperatures referred to was not new to him. He wished to be allowed to refer to one instance, which was, he thought, of a very convincing character. There was a time when artillery was made of cast iron, and this was succeeded by a time when it was made of tubes reinforced by envelopes formed of wrought-iron coils welded together. The transition from that mode of constructing artillery to the modern system of the use of steel to the exclusion of all other metal was a gradual one, and those who had been in the habit of making envelopes by the welding of iron coils thought that the best mode of making steel envelopes must be by the use of weldable steel and the welding it into coils. Artillery was made in that way, and he believed with very good results. But in carrying out a series of experiments while the steel on one occasion was under the hammer, the hammer became gagged, or there was some interference to its working, so that when the hammer was got to work again the steel had cooled down to a blue-heat. On striking it a blow at this temperature the whole coil went to powder, crumbling into pieces about the size, it was said, of salmon's roe. Further, engineers knew that one of the advantages of a snapped head, or a machine-riveted head in a boiler, as compared with the old form of rivet made by hand-hammering, was that all the work was put on the rivet while at a good heat;

when, however, a rivet was made by a pair of riveters with hand-hammers the men could not be induced to leave off hammering; they liked to hammer during the time when the rivet was cooling, although it was always believed that hammering a rivet at that stage of heat—an iron rivet even—was calculated to do harm. Steel was not the only metal that had a critical temperature. Muntz metal, for example, if forged perfectly cold, could be made into a bolt like the one produced; and if forged hot at a proper heat it would be like the second one produced—both excellent forgings; but if it were attempted to forge at an improper temperature, the work would crumble to pieces as shown by the sample. The samples were lying on the table. He had asked Mr. William Anderson to prepare them and speak of them. He had prepared them, but had forgotten to mention them, and he had therefore brought them under the notice of the institution. He thought also he was right in saying that zinc had a critical temperature at which alone it could be safely worked. So that in other metals besides steel there was a critical temperature which must be observed to work with safety. Dr. Pole had referred to instances in which wire had borne great tensile strains, and had preserved that which he had called a high elastic limit, nearly up to breaking-point. Sir Frederick Bramwell confessed that he did not know what elastic limit was. There had been a discussion in that room before about that question in regard to artillery, and he had pointed out that all the complex mathematical formulas introduced into the paper, to ascertain what was the maximum tension that might be used without interfering with the elastic limit, had started upon the supposition that the wire to begin with was a straight wire. Whereas the fact was, as was very well known, that wire, in the very act of its manufacture, was pulled through the "triplet" by which it was formed in the draw-bench, by winding the wire upon a drum, the revolution of the drum pulling it through, and thus the wire, as delivered from the hole, was instantly subjected to an amount of bending which put it into a permanently curved form, and that wire, therefore, ought to be rejected if the passing of the elastic limit was of so much importance as was supposed; because clearly when a straight wire was bent into a circle, and retained that circular form, the elastic limit must have been passed. With respect to the mode of bending specimens of steel and other metals, it had been thought by those who had drawn up the rules for the tests of steel for the construction of artillery (drawn up primarily by Mr. William Henry Barlow, Sir Frederick Abel, and himself) that it was very undesirable to continue the use of the hammer-test for bending. They never knew exactly how the steel was being treated, and therefore they determined that all bending-tests should be made by means of a semicircular ended presser pressing the specimen through an aperture with rounded sides, doing it at a definite rate and with absolute certainty. Unless that was done they could never be

quite satisfied in comparing one set of bendings with another. It was quite true that when a man passed the whole of his time, as prior to that arrangement men sometimes practically did, in hammer-testing strips of steel all of one size, he acquired an amount of skill which was very likely to produce a fair result; but if one test was made, say in Woolwich, and another in Newcastle, there might be two different men using the hammer in different ways; and it was to meet difficulties of that kind that they had thought it expedient to adopt a mechanical mode of bending.

CORRESPONDENCE.

Mr. DANIEL ADAMSON observed that the paper was wanting in this particular, namely, that it took no cognizance of what was alloyed with the iron. The only difference in all the irons in the world arose from the alloys with which the metallic iron ore was combined. This much he could say, in proportion that the alloying element was increased so was the infirmity of such metals at color heat augmented. Blue heat certainly was not an exposition of the condition, as it was manifested in several colors, and the higher alloyed metal would break up at black heat. In a paper which he had read at Paris, "On the mechanical and other properties of iron and mild steel,"[1] he had given in an appendix the chemical compositions, which materially assisted in understanding the infirmity of alloyed metals. The greatest practical importance he attached to color heat was relative to railway axles, in which the heat caused the grease to boil off. The temperature then was dangerous, and such axles subjected to concussions were liable to be broken. The fly-shafts of marine engines could only be protected, in case they became overheated, by stopping the engines and allowing the shafts to cool. Cooling them suddenly in cold water was sure to set up local cracks, and to end in complete rupture.

Mr. T. ANDREWS observed that the interest of the paper would have been enhanced had the author given a complete chemical analysis of the various steels and wrought iron employed, and also ascertained the exact temperature conditions of the metals at the time of observation in each experiment. As regarded wrought-iron, his own practical experience generally coincided with the author's experimental observations—neither iron or steel ought to be worked at a blue or black heat, or considerable injury might ensue. It appeared from a paper "On the modification of tensile strength and ductility which iron and steel undergo when heated to between certain temperatures," read by Mr. E. Cornut before the seventh congress of engineers-in-chief of the Association of Proprietors of Steam Boilers, held at Bordeaux,[2] and from the observations of Mr. C. Huston, Mr. C. Walrand, and others therein alluded to, that there was a peculiar modification of tensile strength and ductility in iron and steel at temperatures ranging from about 572° to 662° Fahrenheit. These researches and the author's experiments might therefore be advantageously considered together, and reference might also be made to a paper by Mr. W. Worby Beaumont, Assoc. M. Inst. C. E., "On modern steel as a structural material."[3] The strength of both iron and steel

[1] The Journal of the Iron and Steel Institute, 1878, p. 383.
[2] Iron, May 1, 1885.
[3] Society of Engineers, Transactions for 1880, p. 109.

was undoubtedly materially influenced in a variety of ways by temperature conditions; low temperature increasing the tensile endurance to some extent, but reducing the power of resistance to sudden impact. Mr. Andrews was engaged in investigating some of these conditions, and the effects of varied temperature on the strength of large forgings in an extensive research. Steel was a complex crystalline chemical substance, and did not appear always to manifest that complete homogeneity and uniformity of structure and behavior which some persons considered it to possess. The elaborate research, "Théorie cellulaire des propriétés de l'acier," communicated in 1885 to the French Academy of Sciences by Messrs. Osmond and Werth, of the Creusot Works, afforded some valuable indications of the peculiar internal structure of steel. Other of its variable properties when under corrosive action in sea-water had been electrically considered in some of Mr. Andrews' recent papers.[1] Mr. Osmond had communicated with him stating that the interchanges of electro-chemical positions of steels in sea-water, and the mode of their corrosion, which Mr. Andrews had observed, had tended to confirm electrically the experimental views he had expressed as to the cellular structure of steels. The microscopic researches of Dr. H. Clifton Sorby, on the structure of steel, also further greatly assisted in affording information on the physical structure and nature of steels, an apprehension of which was essential to a correct prognosis of the ultimate suitability and permanency of these newer metals for structural purposes. Owing to its compound crsytalline character steel might, perhaps, be regarded as more liable to injury from varying temperatures than wrought iron, especially in circumstances where inequality of expansion and contraction might obtain. Mr. Isherwood, of the United States Navy, had observed that steel shafts were liable to become brittle under certain conditions of high temperature, which practically might arise in the case of heated necks or journals. He remarked "that the increase of temperature referred to increases the brittleness of iron and steel, and of steel more than iron, as it accounts for many fractures impossible to otherwise explain of marine engine shafts." Again, "steel shafts are much more subject to these accidents than iron ones, and at high temperatures the brittleness of steel due to temperature is greater than that of iron." The treacherous after-behavior of steel in some instances, such as the failure of steel steam-boilers recently recorded by Mr. Maginnis,[2] and the disastrous sudden fracture of the steel locomotive crank-axle at Penistone on the 16th of July, 1884, which had only been in work about fourteen months, even where it passed satisfactory tensile tests, &c., could not always be accounted for on the author's supposition of temperature injury during manipulation. Moreover, steel generally was liable to develop growing internal

[1] Transactions of the Royal Society of Edinburgh, vol. xxxii, Part I, p. 205. Minutes of Proceedings Inst. C.E., vol. lxxvii, p. 323; vol. lxxxii, p. 281.

[2] "The Engineer," 1885.

flaws, possibly emanating from some of the unevenly distributed nuclei of its crystalline formation.[1] These growing flaws, which, owing to the homogeneity of its nature were always more or less liable to develop in steel, constituted a serious source of unknown danger considered by "L" in his letter thereon, in "The Engineer," December 25, 1885. Such flaws seemed at present undetectable, and generally with steel no intimation was afforded previous to the occurrence of fracture, the behavior of iron in most instances being the reverse in this respect. These circumstances showed that there was considerable risk in the use of steel for many purposes, and although steel primarily endured a higher tensile strain than wrought iron, it did not necessarily follow that this was an infallible indication of its universal applicability, after endurance, or permanent stability under all conditions.

Sir HENRY BESSEMER cited one special case (which came under his personal observation) in support of the views put forward in the paper, and he agreed with the author in believing that many of the so-called "mysterious properties of steel" owed their peculiarity to having been worked at a "half-heat." On one occasion he had seen a plate, which rolled like copper at a white heat, go to pieces in the act of rolling when it was continued after the plate had ceased to be visible in the dark; and it was found that these broken pieces could be rolled when quite cold without fracture, and were perfectly tough, strong metal when annealed. This was an extreme case and was, he had no doubt, largely the result of an overdose of phosphorus in the metal of which the plate was made.

Mr. J. COLVILLE, while generally approving of the paper, must take exception to the remarks with respect to the conduct of mild steel after having successfully withstood hot bending, even if at a blue heat, as he certainly did not think such "almost sure to fly to pieces when cold;" neither could he agree as to plates suffering by being laid upon each other immediately after being rolled.

Mr. A. COOPER regarded the subject of the paper as a most important one, both to the makers and to the users of steel. He was sure if it were better understood and appreciated by ship-builders and boiler-makers much less would be heard of the mysterious crackings of steel plates and bars, as from numerous instances which had come under his knowledge he believed that fully one-half of the plates and bars that cracked in the hands of the users whilst being worked, or after being worked, "hot," as they termed it, failed solely because the working had been continued after the temperature of the piece had fallen below dull red; as subsequent testing had proved in almost every instance that the steel had been not only chemically correct, but also that it would stand almost any amount of punishment either when red-hot or when quite cold, but at the medium temperature, probably when passing through the "blue heat" period, it had been unreliable.

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[1] The Journal of the Iron and Steel Institute, 1885, p. 272.

Mr. F. W. Dick observed that there was very little that could be taken exception to, and very little to add to the paper. It was a matter for congratulation that the author so strongly advocated the working of steel when red-hot, or when cold only. The Steel Company of Scotland had for many years endeavored to impress upon the users of steel that on no account must it be worked at a blue heat. Another important point noticed by the author, and one which should be kept well in mind, was that all steel which had been worked at a blue heat should be subsequently annealed; it should be brought to a full red heat. The manner of cooling, if not too rapidly effected, was not of so much importance. The author had referred to the apparent effect on "medium-hard" steel, of long continued exposure to blue heat. It would be well not to give too much credence to this until the circumstances were fully known. Probably the test-pieces were "strained," by shearing or otherwise, before being placed in the core-oven. The blue heat was insufficient to relieve such strains. It would be noticed that the test-pieces had seemingly been sheared. Plates were constantly stacked hot in rolling-mills, and any brittleness induced in the manner surmised by the author could not have escaped notice. The points to be remembered in this connection were:

1. Initial strains existing in steel were not eliminated by raising to a blue heat. The heating must be continued to full redness before such strains were got rid of.

2. Steel strained at a blue heat and allowed to cool continued in a state of strain, and was much injured, and this injury was much greater than if the steel had been strained while cold.

3. Steel which had been injured by strain at a blue heat was restored to its original condition by raising it to a red heat and allowing it to cool.

Some six years ago experiments were made at the works of the Steel Company of Scotland to determine the influence of manganese on the behavior of steel at the blue heat. Wide variations in the quantities of manganese present (from 1 per cent. to 0.2 per cent.) produced no apparent differences in the results, and this led to various brands of iron plates being subjected to similar tests. It was needless to add that the iron plates were similarly affected at the blue heat. The presumption was that manganese was not the cause, although it might have some effect upon the brittleness of steel at this temperature.

Mr. Thomas Gillott observed that the tenderness of iron at a heat below redness had been known in the best Yorkshire iron works for probably more than fifty years, and "blue-shortness" was a recognized weakness. So far as his experience went steel was more troublesome in this respect than iron, but it was scarcely correct to attribute failures generally to working either iron or steel after a red heat had been passed. Nor need many of the cracks often heard of be called mysterious if the history of failing plates was fully traced from the ingot onwards. Contraction

cracks in steel ingots were often caused by the irregularities of a worn-out ingot mold; and some cracks could be welded up when hammering, but would always leave a line of weakness across the finished material. A slight red-shortness might leave a flaw when hot that would prove a weak spot when cold in a plate that would generally prove on testing as ductile as could be wished. One condemned by him after rolling had slight cracks across in one part of a plate 14 feet by 6 feet 10 inches by $\frac{5}{8}$ inch, and although so ductile that it was scarcely possible to break shearings after heating and quenching, there was a line of weakness due to red-shortness across the plate that doubtless would have fractured in work had it been used. An analysis of the red-short part gave carbon 0.120, silicon trace, sulphur 0.041, phosphorus 0.068, manganese trace. The chief trouble generally arose with large plates, such as those used for the fronts of marine-boiler furnaces, and requiring to be flanged for the boiler-shell and furnaces. Could these plates be flanged in a press at one heat no doubt less would be heard of failures in steel, but taking the results of his own experience in a total number of about 2,500 furnace front plates for marine boilers there were nearly 900 different patterns, so that, for commercial reasons, flanging in successive heats and straightening at a single heat afterwards could scarcely be avoided. Could marine engineers agree on standard patterns better methods of flanging than those generally in use might be adopted. After straightening and facing the flanges slight adjustments of the flanges to the extent of not more than one-fourth inch had to be made, because the time during which plates would remain hot enough for working, when not overheated, is not more than five to ten minutes, depending on their thickness. These adjustments were generally effected by applying heaters and setting by hand hammers, or, in other words, performing the work in the precise way objected to by the author. The heaters were not applied "to take the chill out," as they were just as much used in summer as in winter, but to enable the work to be done; and as many flanges for attachment to the shell of a marine boiler were as much as 9 inches deep for a plate $\frac{7}{8}$ inch thick, some method of adjusting had to be adopted which would locally heat a plate for a slight adjustment, without causing inaccuracies in parts already correct. Any annealing after such an operation would simply entail a further adjustment, as it was hardly possible to draw a large flanged plate out of a furnace without distortion, and it rarely happened that such plates were flat when allowed to cool freely.

There was little doubt that more plates were adjusted by applying heaters, instead of again using the furnace, than was absolutely necessary, but it was open to question whether two or three furnace-heats did not damage iron or steel more than applying heaters after one furnace-heat. No accurate record was kept of fractures in Yorkshire iron when adjusted as described, but he thought that not one plate in twenty would be cracked by setting or riveting, and such cracks, when there

were any, would nearly always occur where parts had been somewhat overheated in the furnace. The difficulty of obtaining perfectly uniform heat in a furnace 16 to 18 feet long and 8 to 10 feet wide, with a large semicircular plate having some flanges 9 inches and others 5 inches deep, occupying the available length and breadth of the surface, would be recognized by those having experience in such work, so that it was by no means easy to secure sufficient heat all over it without a slight excess in some part. Work of this kind could not be treated like the small strips used by the author, and his own impression was that iron was less damaged by blue heats for setting than it would be by additional furnace-heats.

As regarded mild steel, his experience led to the conclusion that it was more liable to fail at a blue heat than best iron. Frequently, when through the drawing in of the unflanged edge of a furnace front plate while being flanged, a furnace-hole had been oval to the extent of ¾ inch, and a stretcher-bar had been put across the shorter diameter whilst the plate was hot for straightening, so as to obtain greater roundness when cold, by the short diameter contracting on the stretcher, and without injury in the case of iron, but not so with steel. As an instance of this, a mild steel plate for the furnace front of a boiler, 12 feet 3 inches in diameter, with two furnace openings of 3 feet 6 inches, had a stretcher-bar placed across the vertical diameter of one furnace that was slightly oval, and it cracked the flange through at the upper point, although the distance between the upper unflanged edge and the inside of the furnace opening was only 5½ inches. Yet this plate, which was full 11/16 inch thick, when tested showed a tensile strength of 28.8 tons per square inch, and elongated 25.625 per cent. in 10 inches, the contraction of area being 42 per cent., and the fracture silky. The strips when heated and quenched were bent to 1½ inches between the folds, and the carbon sampled 0.15 per cent.

With respect to the composition of steel, his own experience appeared to indicate that if mild steel contained from 0.10 to 0.15 of phosphorus, although capable of sustaining severe bending-tests as it left the rolls, it tended to become brittle by successive heats, far more so than steel in which phosphorus was low. The radius of the curves to which the specimens were bent was also of great importance, but was probably kept uniform, although this was not easy.

Mr. J. C. Hudson hoped to be able to indicate a direction in which experiments might be made to find the reason why steel was difficult to work at a blue heat, and more so than when hot or cold. He would consider briefly plate-steel, such as was used in boiler-making, bent when cold, when red-hot, and when at a blue heat. In bending a piece of steel plate from straight to a curve, the outer surface of the curved plate would be stretched, and the inner surface compressed. But as the power of steel when cold to resist a tensile strain was usually considered less than that required to compress it, it might be supposed the plate would be distorted by stretching more than by compression, in

order to accommodate itself to the altered condition of a plate when straight and when bent; and as steel had a great range of elongation under a tensile strain before breaking, evidently a plate might be bent a number of times before fracture. The elasticity of steel would also aid this plate in resisting fracture. Steel when red-hot, or at a higher temperature, would stand bending, as the metal then was more plastic, and the power to resist compression was considerably reduced, and when bending it the outer surface of the curve might be stretched, but the inner surface would be compressed, which compression would reduce the stretching on the outer surface, and considerably reduce the fatigue on the metal. But suppose that steel at a blue heat had lost the power of elongation, or had it considerably reduced, and that the power to resist compression remained practically the same as when cold, then it would appear that a plate bent under this condition of temperature would be rapidly fatigued on the outer surface of the curve, and a fracture would follow owing to the metal being then unable to resist the strains consequent to cooling. From the foregoing he ventured to suggest that the reason why steel was difficult to work at a blue heat could be readily investigated by experiments on the metal at this temperature, and under tensile strain, when he believed it would be found to have lost to a great extent its power of elongation, when also its power to resist compression was not much reduced. His observations of the fracture of steel worked at this temperature confirmed him in this view.

Prof. D. E. HUGHES observed that if blue heat did affect the strength of steel, then it could only be through some molecular change in its structure, and this could be easily determined by testing its magnetic capacity. The day must soon come when a sample of iron or steel would no longer be broken or destroyed in order to find out the molecular condition of its structure. Electricity or magnetism should be able to do this. He had already shown, in his induction balance, that by its aid the slightest change could be detected in the molecular structure of iron and steel, and any strain or flaw be at once found out, and he had shown that, when a current of electricity was sent through a wire or bar of iron, a very slight mechanical strain reduced the self-induction of the bar 40 per cent. It was by physical means alone that the hope could be indulged of penetrating inside a bar of iron or steel, and the exact molecular structure be revealed at any given point or after any given treatment; and although at present no practical instrument existed suitable for tests on all shapes and sizes of iron or steel, the day must come when the present almost brutal method of testing iron and steel would give way to more scientific and far more reliable tests than could be obtained without the aid of physical means. He had tried to solve this question—he was trying still—and others would succeed, even if he failed, as the problem had been already solved from a scientific point of view. Needing only a practical instrument which

could do in rough usage and on any scale that which could at present be accomplished with comparatively small samples.

Mr. C. P. SANDBERG remarked that the alleged great advantage of the use of steel instead of iron, in effecting a saving of 25 per cent. of fuel in steel boilers, through the possibility of using higher pressure, was enough to prove its progress, but it would necessarily be checked by this unexplainable phenomena of cracks, snapping, and fractures, as stated in the paper. It had been the same on the introduction of steel rails, but this has now almost disappeared, and Bessemer steel for rails was now produced with great regularity. This, and the absence of fractures, had been principally arrived at by reducing the hardness, though at the expense, unfortunately, to some degree in their wearing qualities; and he anticipated that if a demand should now arise for harder steel rails there would be some increase of fractures. It would be of great importance to treat this hard steel more carefully than mild steel, by finishing the working or rolling it out before it had declined to a blue heat, and then cooling it slowly and regularly without cutting it when cold. In fact, it should always be borne in mind that the less steel was touched after leaving the rolls, the less was the probability of spoiling it, and this applied especially to hard steel. Mr. Sandberg strongly advocated the employment of chemical as well as mechanical tests in the manufacture of steel rails, and that the latter should be subjected to the same conditions of trial as they would be exposed to in actual practice. He much regretted that there was a total absence of chemical tests to represent quantitively what was meant by mild, very mild, and hard steel. The author should have given, in conjunction with the mechanical tests, the constituents of the various samples of steel, including not only the carbon and silicon, but the other constituents, such as phosphorus, sulphur, and manganese. If makers and inspectors could afford to do this for steel rails, they could surely do so for steel plates. He was not prepared to say that the chemical results would in all cases explain abnormal or curious features developed by mechanical testing, but they would in most instances explain matters, and thus give the key to correction. The chemical testing of different productions, obtained by different processes, was of great value as a guide; but serviceable steel might be had of variable composition, therefore he did not approve of specifying for steel of one fixed chemical composition only. He should like to learn from the author how the temperatures defined as blue and black heat had been taken.

Where bad results, such as snapping and fractures, occurred in steel, it was no easy matter to decide whether such behavior was due to bad steel or to bad workmanship; for improper treatment could spoil even the best steel; and it was in this view that the paper was of great practical interest to all users of steel plates. However, the results of his inspection of steel rails, and which might also apply to steel plates, tended to show that regularity of production was rarely if ever obtained from a new maker at the first start, from want of experience

both of the machinery and of the mill, which was not got in a day; and this might explain occasional bad results; but it should by no means be disheartening, for it would soon disappear, and was almost unavoidable. As regarded a statement that inferior German steel had been used for bayonets supplied to the British army, he thought the saving in first cost of an article like that was wrong. Such articles should be made solely of Swedish steel, one of the peculiar advantages of which was a high degree of "body," or a capacity to stand many repeated heatings without becoming soft.

Mr. H. SHARP stated that the injurious effects of working steel at a blue heat had been well known to many steel makers for some years. An elaborate series of experiments on this subject had been made by Mr. J. F. Barnaby, Admiralty overseer at the Cyclops Works, Sheffield, the results of which had been published by the admiralty,[1] and he felt that he could not add more to the subject than had been published by that gentleman. There was one point, however, which he thought it might be well to mention, namely, the practice which was carried on in many boiler yards of making steel plates hot preparatory to bending them in plate-bending rolls. A long experience has satisfied him that this process was most injurious to steel, and he had no doubt that many of the unsatisfactory results given by steel boilers might be attributed to this cause.

Mr. F. W. WEBB submitted the following particulars of bending-tests and tensile tests made in the testing departments of the Crewe Works of the London and Northwestern Railway:

I.—*Bending tests on strips sheared from one ordinary annealed boiler-plate* (62–12 V).

[Carbon, 0.19. Bent through angle of 135°, forming where bent angle of 45°.]

No. of pieces.	No. of times bent and straightened.	Remarks.
2	2	Two strips sheared off plate (not reannealed. Broke during second straightening.
3	7⅞	Three pieces made red-hot and tested in that condition. Broke during eighth straightening.
3	1	Annealed and made blue-hot, and bent in that condition. Broke during first straightening.
1	Nil.	One piece hammered at a blue heat. Broke during attempt to bend at a blue heat.
2	Nil.	Hammered at a blue heat and allowed to get cold. Broke during attempt to bend.
2	2	Hammered at a blue heat and then annealed; bent cold. Broke during second straightening.
2	3	Pieces of the ordinary plate simply annealed and bent cold. Broke during third straightening.

[1] "Influence of temperature on the strength and ductility of steel and iron," 1881. "Effects of heat on the bending qualities of iron," 1881. "Effect of repeated heating and cooling on tensile strength of steel and iron," 1882. [Inst. C. E., Tracts, folio, vols. 29, 32.]

II.—*Experiments on the tensile strength of boiler-plates tested under the conditions stated below.*

No. of pieces tested.	Breaking weight per square inch.	Extension taken on a length of 10 inches.	Remarks.
	Tons.	*Per cent.*	
3	30.96	23.6	Ordinary plate. Annealed and tested cold.
1	38.04	4.3	Bent three times while blue-hot to angle of 30°, and tested cold.
2	35.24	10.05	Hammered while blue-hot. Tested when cold.
2	30.26	23.8	Hammered while blue-hot, and annealed. Tested cold.
3	31.95	7.5	Bent once when blue-hot. Tested cold.
3	30.74	22.1	Bent once blue-hot. Annealed, and tested cold.
3	31.65	22.5	Bent red-hot. Tested cold.
3	32.42	12.5	Annealed plates. Bent, and tested cold.
3	30.60	23.63	Annealed plates. Bent, reannealed, and tested cold.
2	38.80	11.4	Tested while blue-hot.

From the bending tests it was obvious that plates which had been hammered, bent, or rolled while blue-hot were very liable to fracture when afterwards subjected to bending or concussive action. The annealing appeared to restore the ductility to a plate which had been worked blue-hot, to a great extent, but not completely. The bending was performed by gentle blows from a steam-hammer, and the angle 135° was selected merely for convenience. None of the plates under tensile strain broke across the place where they were bent. The main effect appeared to be on the elongation of the plates, which was much reduced by hammering and bending either when blue-hot or when cold, though in each case it was nearly restored by annealing. The increased tensile strength of the plates tested blue-hot was remarkable. The plates also stretched by fits and starts. The elongation was less and the strength greater than ordinary plates. All the tests, both bending and tensile, were from the same boiler-plate (62–12 V).

*** The results of a series of experiments, undertaken by Mr. W. Parker, M. Inst. C. E., at the suggestion of a member of council, on the question of steel becoming brittle when worked at a blue heat, will appear in a future volume.—Sec. Inst. C. E.

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